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AN

ESSAY

UPON

SINGLE VISION WITH TWO EYES:

TOGETHER WITH

EXPERIMENTS

AND

OBSERVATIONS

ON

SEVERAL OTHER SUBJECTS IN OPTICS.

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By WILLIAM CHARLES WELLS, M. D.

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## ESSAY

UPON

### SINGLE VISION WITH TWO EYES.

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ON

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AN  
E S S A Y

UPON

SINGLE VISION WITH TWO EYES.

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P A R T I.

*Of the different Opinions concerning single Vision with  
two Eyes; and principally of those of  
Dr. Smith and Dr. Reid.*

THE end I have chiefly in view, in this  
Essay, being to offer a new solution of the  
question, why objects are perceived single  
with two eyes, I think it incumbent upon  
me, in the first place, to show, that none of  
the opinions I have met with upon this  
subject, can be admitted as just.

These opinions, or such of them at least  
as have gained any considerable reputation,

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may

may be reduced into two classes. The first comprehends those of Galen, Alhazen, Rohault, Dr. Briggs, and Sir Isaac Newton, all of whom have regarded the question I have mentioned as equivalent to the following one: Whence comes it, that the mind should be affected with only one perception from two impressions upon the external organs of sight, since either of those impressions is, of itself, sufficient to produce a similar perception? Their universal answer has been: Because the two impressions are united before they are communicated to the mind. And the only difference among these authors, has been with respect to the manner in which such an union takes place. To the second class are to be referred the opinions of those, who hold it as certain, that an object is seen single by both eyes, because it is seen by each of them in the same external place; and who profess to point out some law, or constant rule of vision, from which this sameness of place is to be derived as a necessary consequence. Aguilonius, I believe, first gave  
 this

this view of the question, which has since been adopted by Dechales, Dr. Porterfield, Dr. Smith of Cambridge, and Dr. Reid of Glasgow.

In <sup>opposition to</sup> ~~the~~ the opinions of the first class, more especially as they have been repeatedly examined by others, I think I need only say, that they must all be considered as mere conjectures, founded upon certain supposed changes in the brain and nerves, the existence of which it is impossible, from the nature of the parts, either to demonstrate, or to refute by experiments; and that no one of them, though admitted to be true, is yet sufficient to explain the phenomena on account of which it was framed.

The opinions of the second class being built, as their authors think, upon experiments and observations, both allow and demand a more accurate investigation. I shall proceed, therefore, to examine such of them as I am acquainted with, beginning with that of Aguilonius; and what I shall observe concerning it will apply also

to those of Dechales and Dr. Porterfield, who have done little more than copy what he has said.

If a line be drawn through the point of the mutual interfection of the optic axes, parallel to the interval between the eyes, Aguilonius calls it, from its office, the *horopter*; and if through this line, a plane be made to pass at right angles to that of the optic axes, he names it the *plane of the horopter*. After defining these terms he asserts, that, by a law of our constitution, all bodies which we see with one glance or look, whatever are their real places, appear to each eye to be situated in this plane.— And if this be granted to him, he easily and satisfactorily shows, why some should be seen single with two eyes, and others double. For since, according to a second opinion maintained by him, and not contradicted, I believe, by any other writer upon vision, the two lines of direction, in which an object is seen when we employ both eyes, can meet each other only in one point, it follows, that all bodies which are

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really

really situated in the plane of the horopter, must necessarily appear single, as the lines of direction in which any one of them is perceived by the two eyes, coincide in that plane, and no where else; and that all bodies, which are not situated in the plane of the horopter, must as necessarily appear double, since, in this case, the lines of their visible directions intersect each other, either before or after they pass through it.\*

Against the truth of this explanation, only one argument need be offered.—Were the visible places of all bodies to be contained in the plane of the horopter, these would appear of magnitudes proportional to the angles which they subtend at the eye. A finger, for instance, held near to the face, would seem as large as the part of a remote building it might conceal from the sight. But, as this is contrary to experience, the principle from which it is derived, must be rejected, together with all its consequences. To Aguilonius, however, the merit is due of being the first, who  
 so

\* Aguilonii Optica, p. 110, 148, 331, 344.

so far generalised the phenomena of single and double vision, as to observe, that those objects alone are seen single, which are really situated in the plane of the horopter.

The opinion of Dr. Smith is the next in the order of time. \* " If it  
 " be asked (says that author) why in  
 " seeing with both eyes we do not al-  
 " ways see double, because of a double sen-  
 " sation, I think it is sufficient to say,  
 " that in the ordinary use of our eyes,  
 " in which the pictures of an object are  
 " constantly painted upon † correspond-  
 " ing places of the retinas, the predomi-  
 " nant

\* Compleat System of Optics. Vol. I. p. 48.

† Dr. Smith gives the following definition of *corresponding points*. " When the optic axes are parallel or meet in a point, the two middle points of the retinas, or any points which are equally distant from them, and lie on the same sides of them, either towards the right hand or left hand, or upwards or downwards, or in any oblique direction, are called *corresponding points*." Vol. I. p. 46. According to this definition, points correspond which have a certain agreement in situation. No contradiction is, therefore, implied in this system by saying, that an object may appear single, though its pictures should fall upon points which do not correspond. Dr. Reid's definition of the same term is very different.

“ nant sense of feeling has originally and  
 “ constantly informed us that the object is  
 “ single. By this means our idea of its  
 “ outward place is connected with both  
 “ those sensations, as is manifest by its  
 “ appearing in two places when its pictures  
 “ are not painted upon corresponding places  
 “ of the retinas; which is only a direct  
 “ consequence arising from our general ha-  
 “ bit of seeing.” Should any one now en-  
 quire whence it is, that, to produce single  
 vision, all men agree in directing their eyes  
 toward the object in such a manner, as to  
 receive its pictures upon corresponding  
 points of the retinas, since custom might  
 have connected the sensations of any other  
 two points with the information of its unity  
 from feeling :\* This answer may be given  
 in the words of Dr. Smith : † “ When we  
 “ view an object steadily, we have acquired  
 “ a habit of directing the optic axes to  
 “ the

\* This objection is made to Dr. Smith's theory by Dr. Reid,  
 who seems to have overlooked the answer. Reid's Inquiry into  
 the Human Mind, 3<sup>rd</sup> ed. p. 332.

† Vol. I. p. 46.

“ the point in view ; because its pictures  
 “ falling upon the middle points of the  
 “ retinas, are then distincter than if they  
 “ fell upon any other places ; and since  
 “ the pictures of the whole object are equal  
 “ to one another, and are both inverted  
 “ with respect to the optic axes, it follows  
 “ that the pictures of any collateral point  
 “ are painted upon corresponding points of  
 “ the retinas.”

Such is the solution which Dr. Smith has given of this celebrated question, and such the reply, which his general account of vision furnishes to one objection against it. But there are others which, in my opinion, cannot be so easily repelled. Before I offer these however, I beg leave to remark, that although it were proved, as I think it may be, that he is mistaken in the fact of objects appearing single, when their pictures fall upon the middle or other corresponding points of the retinas, still the truth of what is peculiar to him \* of the solution he

\* Dr. Reid attributes to Bishop Berkeley the opinion, that objects appear single to two eyes, from an experienced connection between



he gives, might remain unbroken.— Objects, it may be said, are constantly seen single when we direct our eyes to them in a particular manner. Their pictures must, consequently, in every such case, fall upon the same places of the retinas; and whether these be corresponding or not, the unity of the visible appearances will be owing to the connection, which has uniformly been observed between the sensations of those places, and the information from feeling, that the objects which cause them are single. What I shall say, therefore, upon his opinion, will tend to show, that, admitting the fact respecting corresponding points to be true, his explanation of it ought however to be rejected.

For *first*, it may be observed, that, if we are taught by *feeling* to see objects single,

between particular sensations of sight, and the informations of touch. But I no where find it mentioned in the works of that author; and I even think it probable, that he purposely avoided treating of the question, as he found, that the solution of it, which naturally flowed from his principles of vision, was with difficulty to be reconciled to other conclusions he had derived from the same source.

notwithstanding a sensation in each eye, the informations of the former sense ought to be uniform, or else one set of visual appearances would be associated with different reports from feeling, and no certain mark afforded us which of them we should trust. Now Dr. Smith himself is obliged to confess, that we sometimes *feel* double, “ as  
 “ in the dark, when a button is pressed  
 “ with two opposite sides of two contiguous  
 “ fingers laid across ; for this reason, that  
 “ those opposite sides of the fingers have  
 “ never been *used* to feel one but always  
 “ two things at a time.” \* He adds, “ We  
 “ have learned, therefore, by experience of  
 “ both senses compared together, to make  
 “ their informations consistent with each  
 “ other.” Here then we find him to allow, that feeling is not always the predominant, but sometimes the inferior sense ; that its informations are not constant and original, but changeful and derived ; positions di-  
 rectly

\* Vol. I. p. 48. Dr. Smith however has, from the influence of system, I suppose, mistaken this fact ; for the button is *felt* double, when pressed in the manner above mentioned, though we should not be in the dark, and should even *see* it to be single.

rectly contrary to those he had immediately before maintained. But in the first instance of difference between the informations of the two senses, what rule had we for determining which was the most worthy of credit? How does a blind man correct his errors of touch? If the button be felt double, because pressed by two parts *not* accustomed to feel the same thing at the same time, there must have been a period in the life of every person, when a body pressed by any two parts would have been felt double, by three parts triple, and so on. Nor could sight have corrected those deceptions, if they can be called such; for every thing by the same hypothesis must then have also been seen double. How came we therefore, both to feel and see things single? Surely not by comparing the informations of the two senses together.

But *secondly*; were we to grant, that the sense of touch has originally and constantly informed us that objects are single, it would not follow, that we are thence taught to *see* them also single. For since the place,

which an object seems to either eye to possess, manifestly depends both upon its apparent distance, and its apparent direction from that eye, if visible place be, in the language of Dr. Smith, only an *idea* of real or tangible place, visible direction must bear the same relation to tangible direction ; a consequence of which is, that we can never have a more accurate knowledge of the direction, in which an object may lie from any part of our bodies, by sight than by touch. Facts however prove the contrary. Let any person, for instance, taking a pin in his hand, endeavour, without looking, to bring its head upon a level with either of his eyes ; and there are many chances to one but he will fail in the attempt, of which sight will inform him, when he turns his eye to the object. This to me is a convincing argument, that external bodies are not seen in certain directions, because they have been previously felt in them ; and consequently, that visible place, of which visible direction is a component part, is not merely a representative of the place perceived

ceived by touch. But if the place, in which an object appears to each eye separately, does not entirely depend upon any lesson from feeling, the inference is, that when an object appears in one and the same place to both eyes together, neither is this effect to be attributed solely to the informations of that sense.

*Thirdly*; in whatever direction an object may appear to either eye, it certainly cannot be seen in the same place by both, except at some point common to the two directions. Dr. Smith acknowledges this, and says, \* that when an object is perceived single with both eyes, it is seen at the mutual intersection of the two visual rays; the visible direction of any object coinciding, according to him, with the visual ray, or the principal ray of the pencil which flows from it to the eye. Should we then even allow, that all we know by sight of the places of bodies has been borrowed from feeling, it will still be easy to show, that the rule of vision for each eye, which he  
has

has derived from such experience, that of our seeing objects in the directions of their visual rays, is inconsistent with many of the phenomena of sight with two eyes; and consequently, that he has left unremoved the chief difficulty of his subject, which was to explain the single appearance of objects to both eyes, from those laws, or rules of vision, which affect each of them singly. For it is a well known fact, that if two bodies of the same shape, size, and colour, be placed, one in each optic axis, they appear but as one body, provided they be at equal distances from the eyes. Agreeably to the theory of our seeing objects in the direction of their visual rays, this cannot happen, except the united body appear at the intersection of the optic axes. Dr. Smith accordingly, \* maintains that it does. Now, in the first place, I appeal to experiment for a direct proof that it does not; and in the second, I observe, that, as the two bodies in the optic axes appear as one, whether they be situated within or beyond the

\* Vol. II. Remarks, p. 36.

the concurrence of those lines ; and as a right line joining the bodies, and extended both ways, appears at the same time to the sight as a right line ; it follows, upon admitting the fact which I have denied, that all objects in the plane of the optic axes which are seen in one position and state of the eyes, however near to us, or however remote they may in reality be, must appear to be equally distant, or rather in a line drawn through the concourse of the optic axes, parallel to the interval between the eyes, and named by opticians the *horopter*. Again, if a right line be made to pass through any part of the plane of the optic axes, at right angles to it, the portions above and below this plane are perceived to be in the same right line with the point which is situated in it, and the whole appears perpendicular to the plane. But the point in the plane is seen, by the last article or proposition, in the horopter ; the whole, therefore, of the perpendicular line must be seen in a plane passing through the horopter at right angles to that of the optic axes ; or in other

words, in the *plane of the horopter*, in which consequently all bodies will have their visible places. But this was the very opinion of Aguilonius, to which he was probably led by a similar train of reasoning; though, as a teacher, he might choose rather to ground it immediately upon an original law of our constitution.

It is probable, however, that Dr. Smith did not perceive the conclusions which might be drawn from his doctrine of objects being seen in the directions of their visual rays, since he has nowhere spoken of them. At any rate, it is manifest he did not admit them, as he has mentioned the following circumstance as a fact,\* to which they cannot be reconciled; that, when an object is seen double, both its apparent places are situated between its real place, and the mark at which we look. For, if this were just, together with what he has elsewhere advanced, phenomena ought in many cases to be observed, very different from those which are in truth found to exist. Thus,  
for

\* Vol. I. p. 48.



for example, if a right line be any where placed in the plane of the optic axes, it follows, from what he has said in one part of his book; that those points of it, through which the axes pass, must be seen united at the mark we look at; the axes crossing each other there; and from what I have just quoted, that every other point must be seen by each eye between its real place and that mark. The appearances; therefore, of all the points; if they do not lie disjointed; but are connected together in some orderly manner; will be arranged in the forms, either of two curves, both passing through the intersection of the optic axes, or of four right lines meeting one another at that point. If the right line be placed nearer to the face than the mark we look at, the apparent lines, whether curved or straight, will approach toward us from their common point, but recede from us, if the real line be situated beyond the mark. Such are the phenomena which ought to follow upon the admission of these two parts of Dr. Smith's theory of vision with two eyes, but

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which are not found to exist in nature.—  
 Aguilonius was at least consistent when he maintained, that all objects are seen in the plane of the horopter; while Dr. Smith, by deserting that opinion in part, seems only to have involved himself the more deeply in error.

Having now said what, I hope, will be thought sufficient to show, that the reason given by Dr. Smith, for our seeing objects single with both eyes, is neither grounded on well-attested facts, nor adequate to the explanation of the phenomena observed, I pass to the examination of the opinion of Dr. Reid.

As this neither rests upon, nor includes any new fact in vision, I need only mention, in order to give an account of it,\* that its author maintains with Dr. Smith, that an object is seen in the same place with both eyes, and consequently single, when its pictures fall upon the centres of the retinas, or upon points in them, which are similarly situated with respect to the centres; but  
 differs

\* Inquiry into the Human Mind, c. v. sect. 13.

differs from him in this, that he makes the property to be original, by which any two places in those membranes exhibit only one object, while Dr. Smith derives it altogether from custom. †

In my examination of the opinion of Dr. Smith, I took occasion to remark, that the truth of what distinguished it from all others might remain unshaken, though it were proved, that objects do not appear single, when their pictures occupy any of the corresponding points of the two retinas, since custom might have associated the perceptions of touch, with the sensations of any other parts whatsoever of those membranes. The same observation will not apply with equal justice to the opinion of Dr. Reid. On the contrary, could it be shown, that the places of the two retinas, which repre-

† They differ also with respect to the meaning of a term ; Dr. Smith calling *corresponding points*, such as have the position just mentioned, whether they represent objects single or not ; whereas Dr. Reid says, that those points correspond, whatever their position may be, which represent objects single ; and he appears to me not always to attend to the double use of the same term, when he speaks of the opinions of Dr. Smith.

sent an object single when each receives its picture, are not the centres, or such others as are similarly situated, an obvious inference would be, that the single appearance of the object is not occasioned by a property in those places, bestowed upon them for this special purpose by nature; it being reasonable to expect, that such a property should be found, if any where, in those parts of the retinas which are the most like to each other. I have, therefore, reserved till now, the observations which have occurred to me upon this subject, and which, when stated, must, at least, raise some doubt concerning what has been regarded as true by Dr. Smith and Dr. Reid, and by almost every other writer on vision, since the time of Kepler.

Anatomists have commonly taught, that the centres of the spheres, to which the cornea, the ball of the eye, and the two portions of the crystal line belong, are all placed in the same right line, hence called the optic axis, and that this being produced both ways, passes through the centres of  
 § the

the cornea and retina, considered as surfaces. Opticians, on their part, observe, that an object appears single to both eyes, when the axis of each is accurately directed to it; from which they infer, that the centres of the retinas agree in suggesting but one object, though each receives its picture.— Again; since it is known by experience, that, while any object is seen single, to which the optic axes are turned, others at the same distance from the eyes likewise appear so; and since the pictures of these lateral objects fall upon points in the two retinas, equidistant from their centres, and both upon the same side, that is, both to the right or left of the centres, or both above or below them, opticians conclude, that every two places of the retinas, which are similarly situated with respect to the centres, must also agree in exhibiting but one object, though pictures are received by both.

But the whole of this reasoning is built upon a circumstance in the fabric of the eye, which has been shown by some of the most

most eminent anatomists not to have place. For Varolius \* long ago observed, that the crystalline is not situated in the middle of the eye, but more *inwardly*; and the accurate Zinn † has more lately mentioned, that if the eye be divided into a right and left half, the centre of the crystalline will be found in the inner portion. Haller ‡ confirms this fact; and Winflow's || observation, that the centres of the pupil and iris do not coincide, but that the former is nearer to the nose than the latter, is connected with it; since both Zinn and Haller agree, that the centre of the pupil is placed in the axis of the crystalline, while that of the iris is evidently in the common axis of the cornea and globe. Now, a consequence of this position of the crystalline is, that, contrary to what I believe is universally maintained, no ray of light whatsoever can pass unbent to the retina from the atmosphere, or any other medium differing in refractive

\* Varolii Anatomia, 12mo. p. 16.

† De Oculo, 4to. p. 127.

‡ Elementa Physiologiae, tom. v. p. 403.

|| Winflow's Anatomy, vol. ii. p. 379. English edition, 8vo.

fractive power from the aqueous humour. If, then, the line joining the centres of the cornea and globe of the eye be what is called the optic axis, and if it be true, that objects appear single when we direct both these axes to them, it must be evident, to such as are acquainted with the common rules of optics, that the pictures of those objects do not fall upon the centres of the retinas, but more internally; and, therefore, that the centres and all the other points of those membranes, which by the present system are supposed to represent objects single, do in fact exhibit them double.

It will be said here, perhaps, that the line \* passing from each eye, which we turn to objects when we see them single, is not  
a pro-

\* I am of opinion, that this line, or at least the line which we turn to objects when we see them most distinctly with one eye, is not the common axis of the globe and cornea. For I find, that, when I place the flame of a candle between either of my eyes, and a plane mirror, in such a manner that it may conceal its own image in the mirror from the sight of that eye, or rather that it may be a little below this image, but in the same vertical plane with it, the image of the flame, seen by reflection from the cornea, does not appear upon the middle point of this coat, but upon that point of it which is opposite to the centre of the pupil.

a production of the common axis of the cornea and globe, but some other, disposed in such a manner, that the pictures of those objects are received by the centres of the retinas. I answer; I readily grant the possibility of the thing, but I assert at the same time, that we have no proof of it, which is a sufficient reason for rejecting every conclusion that depends upon its truth.

Admitting, however, that objects are represented single, when their pictures fall upon the centres of the retinas, or upon any other two points which are equally distant from the centres, and both upon the same side, it appears to me, notwithstanding, to be in violation of all analogy, to ascribe this effect, with respect to the points, at least, on the right and left sides of the centres, to any peculiar property which they possess from nature. For when anatomists find, in a new species of animals, organs similar in structure to those of others they are already acquainted with, they immediately conclude, that they are also similar

lar



lar in regard to their use. In animals of the same species, they believe with certainty, that the organs they see in one have the same properties, as the corresponding organs of another; and, if it be possible, they attribute with greater certainty the same properties to two organs of the like kind, which are found in the same individual. Such is the influence of the rule, that resemblance of property is implied by resemblance of structure. Now it is an universal fact, that if an animal be divided into a right and left half, the corresponding parts of those organs, which exist in pairs, are found at equal distances from the plane of partition. Thus, for instance, in respect to the eyes, the two optic nerves penetrate their outward coat at the same distance from this plane. Their muscles, blood-vessels, and every other of their component parts and appendages, are arranged in the like manner; those nearest to the dividing plane, or the innermost, in the one, being similar in structure to the innermost in the other, the outermost to the outermost, and the inter-

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mediate

mediate to the intermediate, It is surely, therefore, natural to expect, that such parts should also be similar in their properties ; and we in fact find this similarity to exist, wherever it can be clearly ascertained what the properties are. Every person, for example, admits, that the internal straight muscle of the right eye performs the same office, with respect to that eye, as the other internal straight muscle does with respect to the left eye. What judgment are we then to form of the opinion of Dr. Reid, which attributes the same original properties, or rather the joint possession of one original property, to places in the retinas situated at unequal distances from the general plane of partition ; which makes an *external* point in one to correspond, in use, with an *internal* point in the other, and this too by a principle implanted by nature ? If such things exist, they may, at least, be said to stand opposed to a most extensive analogy.

To these arguments, *a priori*, against the opinion of Dr. Reid, I shall now add others, which

which are derived from a consideration of its consequences.

*First*; Since visible place, as was formerly observed, includes in it visible distance, it is evident that, if both eyes, by virtue of an original property, see an object in the same place, distance must also be originally perceivable by sight. Dr. Reid, \* however, has himself so ably shown, that we would never have acquired, by means of our eyes, any knowledge of distance, unless they had been assisted by the sense of feeling, that I forbear to say any thing more upon this head, than that the existence of no property can be admitted, which leads to the conclusion I have stated.

*Secondly*; If distance be not immediately perceivable by sight, the only manner, in which an original property of the eyes can affect the visible places of bodies, is by occasioning them to appear in certain directions. Now Dr. Reid maintains, † that every external point is seen in the direction of a line

\* Inquiry into the Human Mind, chap. 6. sect. 3 & 20.

† Ibid. chap. 6. sect. 12.

passing from its picture on the retina, through the centre of the eye. If, therefore, this direction be the same as that suggested by the original property so often mentioned, the latter law is merely another expression for the former, and ought to be rejected as superfluous. If it be different, and should the two laws exist together, objects seen with both eyes might sometimes appear quadruple, sometimes triple, but never single. Were they to exist successively, one when we employ one eye, the other when both, an object, though at rest, should always appear to move when viewed alternately by one and by both eyes ; neither of which conclusions is agreeable to experience.

*Thirdly* ; To show in a different way, and one perhaps more easily understood, that the opinion of Dr. Reid is not consistent with the phenomena of vision it ought to explain, I shall suppose an experiment to be made upon a person who squints. But I must premise, that it appears, from the

observations of Dr. Jurin \* and himself, † that all such persons have one eye of a weaker sight than the other; that when both eyes are open, the weaker is turned away from objects, which are attentively viewed; but that when the strong eye is closed, the weaker is pointed to objects, exactly as the former would be in the same situation; and that it likewise perceives them in similar directions. Let now the ordinary position of the person's eyes, upon whom the experiment is made, be such, that the optic axes intersect each other about an inch or two from the face; and while the other is closed, let the flame of a candle be placed in the axis of the weak eye, which I shall call the left, at the distance of some feet from it, and on the right side of the body. The flame will consequently appear in the same direction, as if his eye had no fault, and will be seen on his right, where it is in reality situated. Both eyes retaining the same position with respect to his head and each other, let the  
 weak

\* Smith's Optics. Vol. 2. Remarks, p. 30.

† Inquiry, chap. 6. sect. 16.

weak eye be afterward shut, and the right opened, and let another object be placed in the axis of the latter, an opaque body being at the same time so disposed, as to hide from it the candle which is in the axis of the left eye. This object in the right axis will consequently appear on the left side. Now, since the two objects, which have been thus viewed separately, are situated, one to the right, and one to the left; and since they have been also *seen* in those positions, their visible places must be two, as well as their tangible, and must be remote from each other. How then should these objects appear, if, instead of being viewed alternately, each by the eye in the axis of which it is placed, they were seen by the two together; the positions and internal states of the eyes being in both cases the same? Dr. Reid must answer; They will possess but one visible place, since their pictures fall upon the centres of the two retinas, points endowed with the original property of representing objects single. But where is this one place

to be found? In the axis of the right eye, or in that of the left, or between the two? In any of these cases, or in any other that can be imagined, the law of visible direction, so much insisted upon by Dr. Reid, that objects appear in the perpendiculars to their pictures upon the retina, and in truth every other law of visible direction hitherto published, must be suspended with respect to one or both eyes; unless, indeed, the united object be referred to the intersection of the optic axes, about an inch or two from the face. This, I believe, Dr. Reid would not readily admit; but if he should, another case of squinting may be imagined, in which the optic axes recede from each other, and where the same reasoning will apply without the possibility of its force being thus eluded. It now remains for me to mention, that the experiment here stated by the way of supposition, in which the optic axes cross each other near to the face, was actually made by Dr. Reid, with this result, that the two objects appeared in different places, when seen by both eyes together;

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and that the other experiment, in which the optic axes are supposed to diverge, was made by myself, with a similar event. Dr. Reid, however, instead of being led, by the termination of his experiment, to impute a fault to the principle from which he had expected a different one, concluded from it, that there was something unnatural, beside the squinting, in the person's eyes, upon whom it was made; though it had been previously ascertained, that objects appeared in the ordinary manner to each of them, when separately employed.

My examination of the second class of opinions, respecting the cause of the single appearance of objects to two eyes, being finished, some person, perhaps, will now say; Granting that no error can, at first sight, be shown in your arguments against those of Dr. Smith and Dr. Reid, is it not a sufficient reason for believing them fallacious, that they prove too much? If objects appear single neither from custom, nor an original property of the eyes, have we not an effect without a cause, and must there  
not



not be something wrong in the facts or reasoning which lead to such a conclusion? The answer I make is as follows: Since visible place contains in it both visible distance and visible direction, it is not necessary that the single appearance of an object, to both eyes, should depend altogether either upon custom, or an original principle of our constitution; for its visible distance to each eye may be learned from feeling, and its visible direction be given by nature; in which case, the unity of its place to the two eyes, will be owing to neither of those causes singly, but to a combination of both; and this I regard as a sufficient reply.

THE END OF PART I.

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UPON  
SINGLE VISION WITH TWO EYES.

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PART II.

*Of a new Theory respecting Visible Direction, and of a Solution hence derived of the Question, why Objects are seen single with two Eyes.*

I NOW proceed to offer a new opinion, why objects are seen single with two eyes ; or in other words, why they appear in the same place to both, this being the light in which I view the fact to be explained.

In every part of natural philosophy, accidents often lead to discoveries, which reason alone might not easily have reached. Under this cover I hope to shelter myself from the charge of presumption, in venturing

turing to give the solution of a problem, upon which the talents of many persons of great learning and genius, have been unsuccessfully employed ; for should I prove more fortunate than such men have been, this must be attributed to the knowledge of a circumstance I observed by chance, in repeating some very common experiments.

The visible place of an object being composed, as I have already several times remarked, of its visible distance and visible direction, to show how it may appear the same to both eyes, it will be necessary to explain, in what manner the distance and direction, which are perceived by one eye, may coincide with those which are perceived by the other : and first with respect to the distance.

In judging of distance by sight, we frequently make considerable mistakes, even when the objects are not very remote ; but no person, I believe, has ever observed, that while an object seemed to one of his eyes at a certain distance, it has appeared to the other to be at a different distance, and from this circumstance alone has been seen double;

or, to express the same thing in another way, that while the visible appearance of an object to one eye, covered the visible appearance of the same object to the other eye, the two appearances did not seem entirely to coincide, and make one, but were seen separate by the two eyes. I do not stop to give the reason of this fact, which must be plain to those who are acquainted with Bishop Berkeley's theory of visible distance; but proceed to mention, that the difficulty in finding a true and sufficient cause for the union of the two visible places of one or two objects to two eyes, must therefore consist altogether in showing, in what manner the two apparent directions may coincide, consistently with the attending phenomena.

Since Kepler's great discovery of the seat and manner of vision, there have been, as far I know, only two theories offered respecting the apparent directions of objects. One is, that they are perceived in the direction of lines passing from their pictures on the retina, through the centre of the

eye; \* the other, that their apparent directions coincide with their visual rays. But both of these theories are inconsistent with the phenomena of single vision with two eyes. For according to neither of them can an object, placed at the concurrence of the optic axes, be seen single, unless we have a most accurate knowledge of its distance; nor will either admit two objects to be seen as one, which are situated in the optic axes, whether on this side, or beyond where they meet, unless the united object be referred by sight to their very point of intersection; both of which conclusions are contradicted by experience. It is evident, therefore, that some other theory of visible direction is required, which shall not be liable to these objections; and such a theory, I hope, I shall bring forward in the following propositions,

\* Mr. D'Alembert has said (*Opuscules Mathematiques*, Tom. I. p. 265) that all optical writers before him had regarded it as an axiom, that every visual point is seen in the direction of its visual ray. But the assertion is not well founded. For Kepler long ago taught (*Paralipomena in Vitellionem*, p. 173) that objects are perceived in lines passing from their pictures upon the retina, through the centre of the eye; in which he was followed by Dechales and Doctor Porterfield; to the latter of whom Dr. Reid improperly attributes the discovery of the same supposed law.

sitions, after mentioning the meanings which I affix to several terms I shall frequently employ.

EXPLANATION OF TERMS.

I. When a small object is so placed with respect to either eye, as to be seen more distinctly than in any other situation, I say it is then in the *optic axis*, or the axis of that eye; and if another small body be interposed between the former and the eye, so as to conceal it, and if a line joining the two be produced till it falls upon the cornea, I call this line the *optic axis*, or the axis of the eye; leaving for future determination the precise point of the cornea it falls upon, or what part of the retina receives the picture of an object which is placed in it.

II. When the two optic axes are directed to a small object not very distant, they may be conceived to form two sides of a triangle, the base of which is the interval between the points of the corneas, where the axes enter the eyes; but if the object

be very distant, then they may be supposed to be two sides of a parallelogram, whose base is the same interval. To avoid circumlocution, I shall call this interval the *visual base*.

III. If there be drawn a line from the middle of the visual base, through the point of intersection of the optic axes, or parallel to them, if they be parallel to each other, I name it the *common axis*. \* This term I believe was invented by Alhazen ; but with him it signified a line drawn from the centre of the junction of the optic nerves, through the middle of the interval between the centres of the retinas.

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\* It may be said, perhaps, that as I do not define the points of the corneas, upon which the optic axes fall, I cannot, with propriety, desire the line which connects them to be divided. To this I answer, that it is not necessary for the purpose I have mentioned, that they should be defined ; if it be granted to me, and I think it cannot be refused, that upon whatever point of the right cornea the right axis falls, the left axis will fall upon a similarly situated point of the left cornea ; that is, if this point of the right cornea be at any given distance from its middle, and upon the inside of it, the corresponding point of the left cornea will be at the same distance from the middle of this, and also upon its inside. Whatever extent, therefore, the line connecting these places of the corneas may have, its middle point will be the same.

Such a line was consequently immoveable. As the term, however, is not in modern use, no mistake can arise from confounding the two meanings, and the reason will soon be seen, why I employ it in the sense I have mentioned. Those who are acquainted with the writings of the older opticians will perceive, that I give it nearly the same signification as they did to their *common radius*.

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### PROPOSITION I.

*Objects situated in the Optic Axis, do not appear to be in that Line, but in the Common Axis.*

EVERY person knows, that, if an object be viewed through two small holes, one applied to each eye, the two holes appear but as one. The theories hitherto invented afford two explanations of this fact. According to Aguilonius, Dechales, Dr. Porterfield and Dr. Smith, the two holes,  
or



or rather their borders, will be seen in the same place as the object viewed through them, and will consequently appear united, for the same reason, that the object itself is seen single. But whoever makes the experiment will distinctly perceive, that the united hole is much nearer to him than the object ; not to mention, that any fallacy on this head might be corrected by the information from the sense of touch, that the card, or other substance, in which the holes have been made, is within an inch or less of our face. The other explanation is that furnished by the theory of Dr. Reid. According to it, the centres of the retinas, which in this experiment receive the pictures of the holes, will, by an original property, represent but one. This theory, however, though it makes the two holes to appear one, does not determine where this one is to be seen. It cannot be seen in only one of the perpendiculars to the images upon the retinas, for no reason can be given why this law of visible direction, which Dr. Reid thinks established beyond dispute, if it operates

rates at all, should not operate upon both eyes at the same time ; and if it be seen by both eyes in such lines, it must appear where those lines cross each other, that is, in the same place with the object viewed through the holes, which, as I have already mentioned, is contrary to experience. Nor is it seen in any direction, the consequence of a law affecting both eyes considered as one organ, but suspended when each eye is used separately. For when the two holes appear one, if we pay attention to its situation, and then close one eye, the truly single hole will be seen by the eye remaining open, in exactly the same direction as the apparently single hole was by both eyes.

Hitherto I have supposed the holes almost touching the face. But they have the same unity of appearance, in whatever parts of the optic axes they are placed ; whether both be at the same distance from the eyes, or one be close to the eye in the axis of which it is, and the other almost contiguous to the object seen through them. If a line, therefore, be drawn from the object to one of the eyes, it will represent all  
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the real or tangible positions of the hole, which allow the object to be seen by that eye, and the whole of it will coincide with the optic axis. Let a similar line be drawn to the other eye, and the two must appear but as one line; for if they do not, the two holes in the optic axes will not, at every distance, appear one, whereas experiments prove that they do. This united line will, therefore, represent the visible direction of every object situated in either of the optic axes. But the end of it, which is toward the face, is seen by the right eye to the left, and by the left eye as much to the right. It must be seen then in the middle between the two, and, consequently, in the *common axis*. And as its other extremity coincides with the point where the optic axes intersect each other, the whole of it must lie in the common axis. Hence the truth of the proposition is evident, that *objects, situated in the optic axis, do not appear to be in that line, but in the common axis.*

Many other experiments might be mentioned which demonstrate the same thing. If, for example, the head of a pin, or of a needle, be interposed between each eye, and any small object to which both the optic axes are directed, the heads of the two pins or needles will constantly appear as one in the common axis. When the heads, however, are near to the eyes, this experiment is not so satisfactory as the former, since, in these positions, they seem as broad transparent shadows, for reasons known to every person a little conversant in optics; whereas the holes appear well defined, though almost touching us. Again; if we hold two thin rulers in such a manner, that their sharp edges shall be in the optic axes, one in each, or rather a little below them, the two edges will be seen united in the common axis, and this apparent edge will seem of the same length with that of either of the real edges, when seen alone by the eye in the axis of which it is placed. If instead of two rulers we employ two strings of different colours, as red and green, the like unity of appearance will

will be observed. But in this experiment it frequently happens, that, contrary to what we might naturally expect, only one of the strings is seen at a time. When, however, only one is seen, its apparent situation is exactly the same as that of the string, compounded, if I may so express myself, of the two when seen together; and hence we have a convincing proof, if any were wanted, that the single appearances of objects must depend upon some law of visible direction affecting each eye, when employed by itself, in the same manner as when it is used conjointly with the other.\*

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\* Du Tour expected, that if two objects of different colours were seen in the same place by both eyes, which however he says, he was never able to observe, the colour of the apparently united object-would be compounded of those of the two really single objects. *Memoires des Savans Etrangers*, Tom. iv. p. 500. And Dr. Reid mentions expressly that it is so compounded. *Inquiry*, p. 293. But in all my experiments upon this subject I have remarked, that, when the two objects appeared united, each was seen, notwithstanding, in its proper colour; the red, for example, appearing as it were through a transparent green, and the green, in the same experiment, as through a transparent red. Nor is there any thing in this inconsistent with the received doctrine of the composition of colours. For in every instance of the production of a new colour, from rays of different colours being at  
same

## PROPOSITION II.

*Subjects, situated in the Common Axis, do not appear to be in that Line, but in the Axis of the Eye, by which they are not seen.*

THE facts which demonstrate the truth of this proposition, are both numerous and common.

same time sent to the eye, these rays fall upon the same sentient extremities of the same nerve. But, in the case before us, the differently-coloured rays fall upon the sentient extremities of two different nerves, which have no communication with each other, except through the medium of the brain. We have greater reason, therefore, for expecting, that the colours impressed upon the two eyes, should be perceived uncompounded, than there is for two colours being perceived separately, which are impressed upon two different parts of the same eye.

From the fact of the two colours being thus perceived distinct from each other, I would infer, by analogy, a mode of argument indeed often fallacious, that if it were possible for us to hear any one sound with one ear only, and another sound with the other ear only, such sounds would in no case coalesce either wholly or in part, as two sounds frequently do, when heard at the same time by one ear; that consequently, if the sounds of one musical instrument were to be heard by one ear only, and those of another, by the other ear only, we could have little or no perception of harmony from such sounds; and that, if in any succession of sounds emitted by one instrument, we were to hear the 1st, 3d, 5th, and so on, by one ear only, and the 2d, 4th, 6th, and so on, by the other ear only, we would be deprived, in a considerable degree, of the melody of such sounds, as this

seems

common. If a piece of wire, or any other substance, representing a physical line, be placed in the common axis, with one of its extremities near to the visual base, and if both the optic axes be directed to its farther or distant extremity, instead of one, two wires will be seen, meeting each other at their farther ends, and gradually diverging as they approach the face, till they apparently terminate at the eyes. If the right eye be closed, the wire which seemed to terminate at the left eye, disappears; and if the left eye be closed, then the other wire disappears; whose termination was at the right eye. The real wire, therefore, in the common axis, appears to the right eye to be situated in the axis of the left, and to the left eye to be situated in the axis of the right, agreeably to what the proposition asserts.

The following experiments will illustrate and confirm both this and the preceding proposition.

seems to depend in a great measure upon a new impression being made upon the auditory nerve by one sound, before the impression of the sound immediately preceding has passed away.

proposition. Through a piece of card, or pasteboard, let two small holes be made, the interval between which is such, that while a very remote object is seen through one of them by the right eye, the same object may be seen through the other by the left eye. Make afterward another hole in the card, or pasteboard, exactly in the middle between the two former; and let the object be viewed through them as before. These, or the outer holes, will now appear one, precisely where the sense of feeling indicates the middle hole to be; while the middle hole will appear as two, which seemingly occupy the places of the real outer ones. The two appearances of the middle hole, which is placed by construction in the common axis, are therefore seen in the optic axes; and as the left is not seen when the right eye is shut, nor the right when the left eye is shut, each appearance is observed in the axis of the eye, by which it is not seen. As I have supposed the distance between the outer holes to be adapted to the interval of the



eyes when they are directed to a very remote object, the optic axes may, in this case, be regarded as parallel to each other. The object, therefore, will still be seen through those holes, though the distance of the card from the eyes be considerably varied; and at all the different distances, the same appearances will be observed, as those which have been mentioned.

Again; take three strings of different colours, as red, yellow, and green, and fasten, by means of a pin, one end of each to the same point of a table. Place now their loose ends in such a manner, that when you look at the pin with both eyes, the visual base being parallel to the edge of the table, the red string may lie in the axis of the right eye, the green in that of the left, and the yellow in the common axis. When things are thus disposed, and both eyes are directed to the pin, the red and green strings, instead of appearing separate, each in one of the optic axes, and inclined to the visual base or edge of the table, will now be seen occupying but one place, either

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together or successively, as was formerly mentioned, and at right angles to the visual base, or edge of the table; in short, exactly in the situation, which the yellow string in reality possesses; and the yellow string, instead of appearing single in the common axis, and perpendicular to the visual base, will now be seen as two, each inclined to the base; that seen by the right eye, apparently occupying the place in reality possessed by the green string, and that seen by the left eye, the place of the red string.

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### PROPOSITION III.

*Objects, situated in any Line drawn through the mutual Intersection of the Optic Axes to the Visual Base, do not appear to be in that Line, but in another, drawn through the same Intersection, to a Point in the Visual Base distant half this Base from the similar Extremity of the former Line, towards the left, if the Objects be seen by the Right Eye, but towards the right, if seen by the Left Eye.*

TWO cases of this proposition have already been proved. For it has been shown  
by

by the first proposition, that objects, placed in the axis of either eye, appear to it to be situated in the common axis. But the common axis is a line drawn thro' the mutual intersection of the optic axes to the visual base, and its termination there is distant, by construction, half that base, from the similar terminations of the axes of both eyes, to the left of the right axis, and to the right of the left. Again, it has been shown by the second proposition, that objects, placed in the common axis, appear to each eye to be situated in the axis of the other; and the terminations of both optic axes, at the visual base, are distant half this base, from the similar termination of the common axis, the left being to its right, and the right to its left.

Let it now be supposed that two objects, one placed in the axis of either eye, the right, for instance, and the other in the common axis, be viewed at the same time by that eye, it is evident that the visible directions of both will be equally removed to the left, from their real positions. But

such an alteration of visible direction, from real position, cannot be imagined to happen, with respect to objects placed in the optic and common axes, unless a similar effect be, at the same time, produced upon such as are situated any where between those lines, or in their vicinity. Facts confirm this: If a line, for example, be drawn through the intersection of the optic axes to a point in the visual base, exactly in the middle between the terminations there of the right and common axes, its apparent situation, to the right eye, will be found to have the same relation to the apparent situations of lines placed in the right and common axes, as its real situation has to the real situations of such lines. And the like will be found, by observation, to be true of every other line, which may be drawn through the point of intersection of the optic axes to the visual base,

The whole of what has here been said may be illustrated and confirmed, by having again recourse to the experiments with strings of different colours, In formerly describing

describing those experiments, I did not mention all the appearances which occurred upon making them, but only such, as had immediate reference to the points then under consideration. When, for instance, a red string was placed in the axis of the right eye, and a green one in that of the left, I said that they both appeared in the common axis. But this is not the only phenomenon to be observed with respect to their apparent number in this experiment. For as the red string is also seen by the left eye, and the green by the right, two other strings become visible, beside that in the common axis, the apparent positions of both of which will be found to be the same with those, which ought to follow from the present proposition. Should now a yellow string be placed between the two former, as in the proof of the second proposition, its appearance to the right eye will bisect the space between the appearances of the red and green strings to that eye; and the like will be true with respect to the appearances of the three strings to the left

eye, agreeably to what the same proposition teaches us to expect.

I believe I need scarcely remark, that, although in most of the proofs and illustrations of these propositions, I have confined myself to the visible appearances of lines *between* the intersection of the optic axes and the visual base, the same things, however, must be equally true of those lines, when they are produced beyond the intersection, with this difference only, that, while the portions within, seem, to the right eye, to be farther situated to the left than they really are, but to the left eye farther to the right, the portions beyond the intersection will seem to the right eye to the right of their real positions, but to the left eye to the left of them. For it is manifest, that, if a line be seen by one eye in a certain direction, a prolongation of it must be seen in the same direction; and that, if a line be made to turn upon any point in itself, the two extremities must move contrary ways.

Should the optic axes be parallel to each other, the same proofs and illustrations will  
still

still apply, since we may here suppose them to meet at an infinite distance from the visual base. In this case, the visible appearances of lines, drawn from this supposed point of intersection to the visual base, will be parallel to the real lines, and distant half this base from them, through their whole extent.

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AS I have thus, I think, sufficiently proved, that the apparent directions of objects are governed by a law, different from any which has hitherto been thought to exist, I shall now proceed to state, in a few words, in what manner the phenomena of single and double vision with two eyes are dependant upon it.

I formerly mentioned, that, since an object is never seen double, merely from its being seen at different distances by the two eyes, the only difficulty in explaining its single appearance consists in showing how its two visible directions may coincide, consistently with the attending phenomena. But we are enabled to do this, with the utmost

most ease, by the theory I have endeavoured to establish. For, if the question be concerning an object at the concurrence of the optic axes, I say it is seen single, because its two similar appearances, in regard to size, shape, and colour, are seen by both eyes in one and the same direction, or, if you will, in two directions, which coincide with each other through the whole of their extent. It therefore matters not, whether the distance be truly or falsely estimated; whether the object be thought to touch our eyes, or to be infinitely remote. And hence we have a reason, which no other theory of visible direction affords, why objects appeared single to the young gentleman mentioned by Mr. Cheselden, immediately after his being couched, and before he could have learned to judge of distance by sight.

When two similar objects are placed in the optic axes, one in each, at equal distances from the eyes, they will appear in the same place, and therefore one, for the same reason that a truly single object, in the concurrence of the optic axes, is seen single.

Here



Here again, as the two visible directions coincide in every point, it is not necessary that the united appearance should be judged to be at any particular distance; that it should be referred, for instance, to the concurrence of the optic axes, where the two other theories of visible direction are obliged to place it, in opposition to the plainest observations.

Objects, any where in the horopter, will be seen single, because their apparent directions to the two eyes will then completely coincide. And for a contrary reason, those placed in any other part of the plane of the optic axes will appear double. To make these things evident, let a line pass through the point of intersection of the optic axes, and any given object, to the visual base, which is to be produced, if necessary; and let it be called the line of the object's real position. Take afterward, in the visual base, or its production, two points, one on each side of the line of real position, and both distant from its termination there, half the visual base. Lines drawn from these

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points, through the point of intersection of the optic axes, must consequently contain the two visible positions of the object. But when this is situated in the horopter, the line of real position will coincide with the horopter, and will not therefore reach the visual base, unless at an infinite distance from the eyes. For which reason, the two lines, containing the visible positions of the object, must fall upon the visual base at a like distance, and must consequently be regarded as coinciding with each other. When the object is not in the horopter, the two lines of visible direction will be found, by the same means, not to coincide.

That I might simplify a matter, which under my management, must, I fear, still be of difficult apprehension, I have, in expressing the law of vision, so frequently mentioned, purposely confined it to objects situated in the plane of the optic axes. But in persons who do not squint, or whose eyes are not distorted by external violence, the two appearances of an object, seen double, are always, either in that plane,

or in some one parallel to it; so that, if the visual base be parallel to the horizon, a line joining the two appearances will, in every case, be also parallel to the horizon. Whoever then is able to explain, why objects in the plane of the optic axes appear either single or double, may readily give a reason for the like appearances of such as are placed any where else. Not to spend much time, therefore, upon this part of the subject, I shall shortly observe, that if planes be supposed to pass through the two optic and common axes, perpendicular to that in which they all lie, and if two lines be drawn from any point of the common intersection of the former planes to the visual base, one along each of the perpendicular planes which pass through the optic axes, these two lines will appear as one, in the perpendicular plane of the common axis; the single visible line, however, possessing the same elevation, in regard to the horizon, as the two real lines: And again, that, if a line<sup>be</sup> drawn from any point of the same intersection to the visual base,

base, along the perpendicular plane of the common axis, it will appear as two, one in each of the planes which pass through the optic axes; the two visible lines having the same inclination to the horizon in their progress to the visual base, as the real single one. In this manner, every thing may be shown to be true, with respect to the single and double appearances of objects without the plane of the optic axes, which has already been done with regard to those placed in it. But farther; since any point, taken at pleasure, in the common intersection of the three perpendicular planes, appears single, the whole of the line of intersection must appear so, and likewise every point of a plane made to pass through it, parallel to the visual base. Such a plane necessarily includes the horizon, and is the same as that, which is called by Aguilonius the plane of the horizon.

To exemplify the principal property of this plane, I shall mention an experiment, which at first I did not understand, though  
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the result was a direct consequence of my own principles. I suspended a fine chord at right angles to the horizon, and retreating a step or two, I looked steadily at a point in it, which was upon a level with my eyes. The chord, in these circumstances, appeared single; but whenever I directed my eyes to any other point of it, either above or below the former, two chords would appear, crossing each other at the part, to which the eyes were directed. In the first case, the whole chord was in the plane of the horopter, but in every other, only that point of it to which both eyes happened to be turned. A conclusion from this experiment is, that no object, which is truly perpendicular to the horizon, will appear to be so, while our bodies are erect, unless we direct our eyes to a point in it exactly upon a level with themselves.

It was once my intention to subjoin here several instances, from the most approved authors, of inaccurate descriptions of the single and double appearances of objects; in order to show, that the theory of  
 visible

visible direction, which I have advanced, is not only consistent with the universally received facts, but that it also discovers to us, some minute errors, which unguided sense has committed upon this subject; it being, perhaps, one of the surest tests of the soundness, as well as one of the chief uses, of theories in philosophy, that they lead to the knowledge of what, otherwise, might have remained for ever hidden. But fearing I have already proved tiresome, I give up this design, and hasten to the consideration of some consequences from my theory, which seem to me both curious and important, and which, when first mentioned, may appear to carry with them their own refutation.

END OF PART II.

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UPON

SINGLE VISION WITH TWO EYES.

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P A R T III.

*Of some Consequences from the foregoing Theory of Objects  
being seen single with two Eyes, together with the  
Explanation of several other Phenomena  
of Vision.*

IT has hitherto, I believe, been thought by opticians, that, if the position of the eye be unchanged, the visible direction of an object will be the same, as long as its picture occupies any one point of the retina; and that, in every different position of the eye; a picture, which continues to occupy the same point of the retina, will represent its  
5 object

object in a different direction. But if the theory be just, which I have advanced in the preceding part of this Essay, neither of those opinions can be universally true. For it follows, from what was there mentioned, that if one of the optic axes be kept fixed, and the other be at different times variously bent toward it, objects, though situated in the fixed axis, will nevertheless change their visible directions, with every variation of the moveable axis; since they must always appear in the common axis, which alters its position with every change of the moveable axis: And again, that, if the two optic axes should vary their inclinations to each other in such a manner, that the common axis, may, notwithstanding, remain fixed, an object placed in either optic axis, and following it in every motion, will possess but one visible direction, in all this variety of real positions. That these conclusions from my theory, or rather parts of it, are true in fact, I can assert upon the authority of observations, and I shall now attempt to  
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trace them both to a common principle, by means of some experiments, which were instituted with a very different view.

When we have looked steadily for some time at the flame of a candle, or any other luminous body, a coloured spot will appear upon every object, to which we shortly after direct our eyes, accompanying them in all their motions, and exactly covering the point, which we desire to see the most accurately. Whatever therefore can be proved concerning the apparent direction of such a spot, in any given position of the eyes, must likewise be true in the same position of the eyes, with regard to the apparent direction of an object, situated at the concurrence of the optic axes; as its pictures must occupy, in this case, the very parts of the retinas, upon the affections of which the illusion of the spot depends. This being premised, I shall now relate one or two observations, respecting the apparent directions of the spot, and consequently upon those of external objects,

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which, as far as I know, have not been mentioned by any other person.

1. The spot is always seen single, whether the surface, upon which it is projected, be touching the face, or at the greatest distance from us; and the reason is plain. For the parts of the retinas, by whose affections from the luminous body it is occasioned, are those likewise which receive the pictures of objects, placed at the interfection of the optic axes; and as such objects always appear single, so must also the spot. The fact indeed is so open to observation, and its cause so easily shown, that I should scarcely have thought of mentioning it, had not Dr. Darwin\* lately told us, that the spot is seen double, as often as the eyes are directed to an object more or less distant than the luminous body which gave rise to it. With respect to our different assertions upon this point, I shall only say, that I have made the experiment, I believe, upward of an hundred times, uniformly with

\* Philosoph. Transact. for 1786, p. 318. Dr. Darwin indeed, says, p. 341, that Buffon had observed the same fact; *but* it is evident he has mistaken that author's meaning.

with the same result; and that, if the spot ever appears double, this must be from some cause very wide of a change in the mutual inclination of the optic axes, to which he attributes it.\*

2. The spot not only appears single in every ordinary position of the optic axes, but cannot even be made to appear double, by any means whatsoever. If it be projected, for example, upon a piece of white paper, whoever makes the trial will find, that, although, on pressing one eye upward or downward, or to either side, the paper will be seen double, yet the spot

\* The only way, in which I think it possible for the spot to appear double, consistently with the universally acknowledged fact, that an object at the intersection of the optic axes is always seen single, is this, that, when the intersection is near to the face, an object placed in it shall not send its pictures to the same points of the two retinas, as it does, when the intersection is more remote. And such I once hoped to find to be the case; for I had formed, upon the supposition of its truth, a more plausible account of the manner in which the eyes are fitted to receive, successively, pictures equally distinct from objects at different distances, than any I had met with. But, after many experiments to ascertain the matter, I was obliged to return to the common opinion, that the picture of an object in the optic axis, whatever be its distance from the eye, is always received upon the same point of the retina.

will always appear single, and to possess its former place on the paper, as seen by the eye, which is not disturbed. Before I knew the result of this experiment, I had imagined, that, the position of one eye being forcibly altered, the external situation of the spot, which was suggested by the affection of that eye, would likewise be altered, and the spot by consequence be seen double. As the event, however, was contrary to my expectation, I began to suspect some cause of fallacy had been overlooked, which at length I thought might be this, that the spot had been seen by that eye only whose position was not disturbed, the violence, suffered by the other, interrupting the due exercise of its functions. To determine, therefore, whether my conjecture was well founded or not, I made another experiment, which is mentioned in the following article :

3. Having looked steadily for some time at the flame of a candle, with *one* eye only, I directed afterward, with both eyes open, my attention to the middle  
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of a sheet of paper, a few feet distant ; the consequence of which was, that a spot appeared upon it in the same manner, as if I had viewed the flame with both eyes, though somewhat fainter. My attention remaining fixed upon the sheet, I now pushed the eye, by which the spot was seen, successively upward and downward; to the right and to the left, and in every oblique direction ; the spot however never altered its position, but kept constantly upon the middle of the appearance of the paper, perceived by the undistorted eye; though the appearance of the paper to the distorted eye, was always separate from the former, and the sheet consequently seen double. My conjecture, therefore, was proved to be ill grounded, and all suspicion of fallacy in the former experiment ~~was~~ ceased.

Now it is evident, from these two last experiments, that the situation of the spot does not depend upon the bare position of the eyes, or else, in the former of them, it would have appeared double, and in the latter,

latter, it would have been moved from the middle of the paper, when the only eye by which it was seen was pushed from its place. Neither can it depend upon the bare position of the muscles of the eye, as these were also moved in the same experiments; nor upon any affection whatever of the optic nerve. For since this last substance is altogether passive, even in those motions of the eyes which do occasion a change of the spot's situation, every alteration, induced upon the nerve by those motions, must be ultimately ascribed to a change of its position; and we have seen, that similar changes of its position have been produced by external violence, without any alteration of the spot's situation. The apparent situation of the spot being, therefore, dependant upon none of these circumstances, and being at the same time affected by the *voluntary* motions of the eye, it must, I think, be necessarily owing to the *action* of the muscles, by which these motions are performed. Assuming then as true, that the apparent direction of an object, which sends its picture to

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to any given point of the retina, depends upon the state of action existing at the same time in the muscles of the eye, and consequently that it cannot be altered, except by a change in the state of that action, I shall proceed to trace to this principle, several phenomena of vision, particularly the uniform singleness of the spot already described, and the two facts respecting the visible directions of objects in the optic axis, which were mentioned in the beginning of this part of my Essay.

The thing itself is universally acknowledged, though a dispute has arisen whether custom or an original property be the cause, that every voluntary motion of one eye, in persons who do not squint, is attended with a corresponding motion in the other. Now as all voluntary motions are produced by muscular action, it follows, that every state of action, in the muscles of one eye, has its corresponding state in those of the other, and that the two are constantly conjoined. When, therefore, the spot appears single to both eyes in their

free positions, the states of action in the muscles must be such, that the direction, in which it is seen by one eye, coincides with that in which it is seen by the other. But, if we push one eye from its place, no change is hereby made in the action of its muscles; for the state of action in those of the free eye is confessedly the same as it was; and it will be attended with a corresponding state in those of the distorted eye; in proof of which it may be observed, that, whenever the pressure is removed, the distorted eye immediately returns to its former position, without the aid of any new muscular effort. The conclusion then is, that, since there has been no alteration in the action of its muscles, neither ought there to be any in the direction of the spot seen by it, which is the fact to be explained.

Hence also is to be derived the true reason, why objects appear double, when one eye is pushed from its place. For as their pictures must fall upon points of the retina in this eye, different from what they  
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formerly possessed ; and as no change is made, by the distortion, upon the visible direction, suggested by any part of the retina, the objects will be seen by the pressed eye, exactly in the same directions as they would have been, *before* it was pressed, had the pictures *then* fallen upon the points of the retina, which they *now* occupy. They must therefore be *now* seen in different directions by the two eyes, and consequently double. An experiment with a contrary event will confirm this explanation, and likewise show more clearly, in what I differ from those who have endeavoured to account for the same fact. Both eyes being open, let one of them be pushed from its situation, and let two similar objects, such as two pieces of money of the same metal and stamp, be afterward so placed, that one shall lie in each optic axis ; these two objects will now appear to be one, and the object so compounded will be seen in the place, to which the undisturbed eye refers the truly single object lying in its axis.

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Another inference from this doctrine is, that, if the eyes are in any very unusual position with respect to each other from the action of their own muscles, as in persons who squint, two objects placed in the optic axes, one in each, will not appear as one object; for each will be seen in the direction, which is determined by the state of action in the muscles of the eye, upon whose retina its picture falls; and as this state, in one eye, does not correspond with that in the other, the directions cannot coincide. This conclusion is verified by the result of an experiment of Dr. Reid upon a person, affected with strabismus, and by that of another, made by myself, both of which have been already related.

To explain, therefore, why an object in the optic axis appears at different times in different directions, though the axis be kept fixed, it is only necessary to show, that, whenever this happens, a change, notwithstanding, occurs in the actions of the muscles which move the eye. With this view, I observe, that the motions of that  
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organ may be divided into two sets; the first, consisting of those, by which one eye is carried along with the other, upward and downward, to the right and to the left, and in every oblique direction, the interval between the pupils remaining constantly the same; the second, of the motions of the pupils, or the anterior parts of the eyes, to and from each other. Supposing now, that both the optic axes are perpendicular to the visual base; should the left axis be afterward inclined to the right side, the natural tendency of the right axis is to incline equally to the same side, so as to preserve its former parallelism to the left. This tendency, however, in the right axis to follow the left, may be counteracted by an effort of the muscles, which regulate the interval of the pupils, until the two axes intersect each other within two or three inches of the face. But it is evident, that the same degree of muscular force will be required to retain the right eye in its original position, as is necessary to give to the left eye its motion toward the right; and

hence, that, in every different inclination of the left axis to the right, an object placed in the latter, though its real position be unchanged, will, nevertheless, appear in a different direction, in consequence of the different state of action in the muscles of the right eye, which accompanies every new degree of inclination of the axes to each other. As the object must always appear in the common axis, the alteration, in this example, of its visible direction, from an *increase* of the mutual inclinations of the optic axes, will be from left to right; but when the inclination decreases, from right to left. If the right axis be the one which is moved, and the left fixed, the alterations of visible direction in an object placed in the latter, from similar changes in their inclinations, will be contrary to those which have just been mentioned.

The reason also can now be made to appear, why an object, preserving constantly its place in the optic axis, may, in a considerable variety of its real positions, possess but one visible direction. For, in such cases,

cases, the change of its visible direction, which might be expected to accompany the motion of the eye in the axis of which it is situated, is prevented from occurring, by a tendency to a change of its visible direction the contrary way, produced by the muscular actions which regulate the mutual distance of the pupils. To know how this happens, suppose the two optic axes to be parallel to each other, and perpendicular to the visual base; and let a physical line be placed in either of them, so as entirely to coincide with it. This line will, therefore, not only be in reality perpendicular to the visual base, but will, in the present state of things, likewise appear so.— Incline afterward both the axes equally to the left side, and it is manifest that the line coinciding, say, with the right axis, must appear equally inclined. Let now the right axis be kept fixed, and the left be carried back again, and its motion continued, until it be as much inclined toward the right side, as itself was just before, and as the right axis is still to the left side; the consequence

quence will be, that the line in the right axis must again be seen perpendicular to the visual base ; for such is the present position of the common axis. Here then we have had two opposite causes of change of apparent direction acting in succession. The muscular actions, producing the joint motions of the eyes, first bent the visible position of a line, in the right optic axis, from a perpendicular to the visual base toward the left ; and the muscular actions, which regulate the mutual distances of the pupils, by *increasing* the inclinations of the axes to each other, moved it afterward, from the left to the right, back again to a perpendicular to the visual base. Let these two causes act together, and it is plain, that no observable effect will be produced by either, as long as they are thus proportioned. When they are not so, only the difference of their forces will be exhibited by the phenomena.

But farther ; to show the extent of this theory of visible direction being dependant upon the actions of the muscles of the eyes,  
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I shall now apply it to the explanation of an instance of apparent motion, which at first may be thought to furnish an argument against it. Look with one eye, the other being closed, at any remote object through a small hole in a card. If you should afterward suddenly attempt to view the hole itself accurately, with the same eye, you will observe both it and the distant object, particularly the latter, to move from left to right, if the right eye be used; but if the left eye be the one employed, then from right to left. Shift now your attention as suddenly back from the hole to the object seen through it, and both will return to the places they formerly occupied. In this experiment, no real change can be supposed to have occurred in the position of the distant object; and had any happened, with respect to either the eye or the hole, the object would not have been seen through the latter. No other fallacy, therefore, exists here, than that things, which are truly at rest, appear, notwithstanding, to be in motion.

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The argument, which I have mentioned may hence be derived against my theory, is this : The visible directions of objects, in the optic axis which remained fixed, were formerly said to be altered, because a new state of muscular exertion was required to keep it so, in every different degree of the inclination to it of the moveable axis. But in the last experiment, there seems no good reason for supposing any change in the inclination of the moveable axis to the other ; for, as one eye is closed, the obvious intention of directing the two axes to the same object, which is, that we may see it single, no longer exists. If then an apparent lateral motion be, in one instance, observed in objects truly at rest, without any change of the interval of the pupils, may not every other motion of the like kind be also independent of the muscular actions, which regulate that interval ?

It is evident, that this argument rests altogether upon the supposition, that in the experiment just mentioned, no alteration occurs in the interval of the pupils. Now,



we may be easily convinced, that some alteration does occur, by applying a finger to the closed eye, which will, by this means, be felt to move toward the nose, when we endeavour to view the hole accurately, and from the nose, when we carry our attention back again to the remote object. Were, indeed, the opinion of Aguilonius \* just, that the mind perceives only those objects distinctly, which are situated at the concurrence of the optic axes, whether they are seen with one or with two eyes, both the necessity and the degree of the alteration would be clearly ascertained. But as this opinion is not just, which I mean to prove from experiments in a succeeding part of this volume, I shall proceed to give another reason, and I think the true one, why the interval of the pupils should be as much altered, when we look with one eye at objects successively, which are placed at different distances, as if we were to view them with both.

It is a fact, for which I have the authority of experiments almost without number,

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\* Optica, p. 84.

though I do not recollect to have seen it mentioned by any author beside Dr. Porterfield, that every change of the mutual positions of the optic axes is conjoined, in persons who do not squint, with a change of the power, in both eyes, to refract the rays of light which fall upon them.— When the axes are parallel to each other, the eyes are in their lowest refracting state; but in their highest, when the axes are mutually intersected within two or three inches of the face; every intermediate inclination being also conjoined with an intermediate degree of refracting power. Now, since those objects are seen most distinctly, the radious pencils from which are accurately brought to points in the retina, it follows, that, although we employ one eye only, the same reason exists for adjusting its refractive power to their distances, as if we saw with both. When, therefore, we view a remote object with one eye, we use it in its lowest refracting state, which, I have observed, is conjoined with the widest interval of the pupils. Should we afterward attempt to

see accurately a very near object, the eye will assume its highest refractive state, and the interval of the pupils be lessened; the consequence of which must be, that both the objects lying in the optic axes will appear to move in the manner already related.

To finish this part of my subject, it seems only necessary to determine, whether the dependance of visible direction upon the actions of the muscles of the eyes be established by nature, or by custom. But facts are here wanting. As far as they go, however, they serve to prove, that it arises from an original principle of our constitution. For Mr. Cheselden's patient saw objects single, and consequently in the same directions with both eyes, immediately after he was couched; and persons affected with squinting from their earliest infancy, see objects in the same directions with the eye they have never been *accustomed* to employ, as they do with the other they have constantly used.

Having thus shown in what directions external bodies are seen, when their situa-

tion with respect to the eye is given, and upon what circumstance the various directions depend, in which a picture upon any one place of the retina can exhibit the object producing it; I should render the theory of visible direction complete, were I now to point out the relative positions of the two lines of direction, in which any two different parts of the retina represent their objects. To ascertain this, the first step must be, to find the place of the retina which receives the picture of an object, whose situation with respect to the external eye is known; and if two such points of the retina were determined, I think the chief difficulty in this matter would then be overcome. But as it appears to me, that the structure of the eye has not yet been sufficiently explained, to enable any person to take this first step, I forbear saying any thing more upon the subject.

END OF THE ESSAY UPON SINGLE VISION WITH TWO EYES.

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EXPERIMENTS *and* OBSERVATIONS

ON

SEVERAL SUBJECTS IN OPTICS.

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ARTICLE I.

*On Visible Position, and Visible Motion.*

**I**N the estimates we make by sight of the situation of external objects, we have always some secret reference to the position of our own bodies, with respect to the plane of the horizon; and from this cause, we often judge such to be at rest, whose relative places to us are continually changing; and others to be in motion, though they may constantly preserve, in regard to us, the same distance and direction. To give  
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in instance, let us suppose our eyes first directed to a star near to the horizon ; should we afterward, by a mere motion of the head, point them to another, some degrees above the former, this second star will appear higher than the first did. Were we now, while the eyes are kept fixed in relation to the head, and the head in relation to the shoulders, to incline the trunk of the body backward, until we bring the optic axes to a third star, this will appear still higher than the second was perceived to be. If instead of directing the eyes successively to different objects, the same object be suffered to remain at the concurrence of the optic axes in all these different positions of the body, it is evident, that it must be seen to move, during the change from one position to another.

The facts I have mentioned are so obvious, that I should not have spoken of them, had I not intended they should introduce the following question : What is there within us, to indicate these positions of the body ? To me it appears evident, that, since they are occasioned and preserved by combinations

combinations of the actions of various voluntary muscles, some feeling must attend every such combination, which suggests, from experience perhaps, the particular position produced by it. But in almost all the positions of the body, the chief part of our muscular efforts is directed toward sustaining it against the influence of its own gravity. Each position, therefore, in which this takes place, must be attended with a feeling, which serves to indicate its relation to the horizontal plane of the earth; and consequently, if our bodies possessed no gravity, or, if the thing were possible, had we been created unembodied spirits, but with the same faculties of perception as we enjoy at present, we could no more have judged one line to be perpendicular, and another to be parallel to the horizon, than we can at present determine, without some external aid, which is the eastern, and which the western point of the heavens. I shall now draw from these principles, the explanation of a fact, which was first mentioned by one of the most ingenious authors that

that have written upon vision, but left by him still to be justly accounted for.

“ I have frequently (says Mr. Melvill)\*  
 “ observed, when at sea, that, though I  
 “ pressed my body and head firmly to a cor-  
 “ ner of the cabin, so as to be at rest in  
 “ respect to every object about me, the diffe-  
 “ rent irregular motions of the ship, in roll-  
 “ ing and pitching, were still discernible by  
 “ sight. How is this fact to be recon-  
 “ ciled to optical principles? Shall  
 “ we conclude that the eye, by the  
 “ sudden motions of the vessel, is rolled  
 “ out of its due position? Or, if it retains a  
 “ fixed situation in the head, is the percep-  
 “ tion of the ship’s motion, owing to a ver-  
 “ tigo in the brain, a deception of the ima-  
 “ gination, or to what other cause?”

I need not, I believe, offer to show, that the fact here spoken of, is not owing to any of the causes Mr. Melvill has specified. I shall therefore, in a few words, point out its dependance upon the principles which have just been mentioned.

It is generally known, I suppose, that when a vessel at sea, in the language of sailors,

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\* Edinburgh Physical Essays, vol. ii. p. 89.





lower extremity, successively backward and forward, to the right and to the left, these motions must, without contradiction, be perceived. Suppose now, our bodies to be similarly inclined with the pole, during its different positions, so as to be constantly parallel to it; it is evident, that its motions will be as readily perceived in this case, as they were, when our bodies were erect; and this is all that happens in the experiment of Mr. Melvill.

Should the necessity of supporting the body against its gravity, by the actions of our voluntary muscles, be suspended in whole, or in part, our judgments of the situation of objects, with respect to the horizon, must become irregular and uncertain, notwithstanding any general habit we may have acquired from experience. An instance of this, I think, I have observed; for I have frequently remarked during a sea voyage, that, when the wind blew so strongly, and in such a direction, as to occasion the vessel to heel, or lean much to one side, chords freely suspended from the

roof of the cabin, and kept stretched by heavy bodies attached to them, have appeared to me, as long as I lay in bed, though they were necessarily perpendicular to the horizon, to decline considerably from that position; while the sides of the cabin seemed, if not perpendicular, at least much less inclined to the horizon than they were in reality. My body being here supported by the bed, I was consequently without those feelings, which indicate its position with respect to the horizon. Objects therefore appeared to me in those situations, in which I had been accustomed to see them. In confirmation of which I may mention, that, when I got up, and stood upon the floor of the cabin, the chords seemed perpendicular, or nearly so, and the sides of the cabin inclined; for I was now obliged to exert a proper degree of muscular force, to keep myself upright. What I ~~say~~<sup>here</sup> say, however, is from the recollection of things observed some years ago, when I had no thought of making the use of them I now do; for which reason,

I may possibly have committed some trifling error in stating them ; but none, I believe, sufficient to affect the theory they are brought to support.

It being my intention to treat, in the present article, of several facts relative to visible position and motion, which seem to me to need explanation, without regarding whether or not they depend upon any common cause ; I pass to the consideration of the apparent rotation of objects, when we have become giddy, by turning ourselves quickly and frequently round.

Some of the older writers upon optics imagined the visive spirits to be contained in the head, as water is in a vessel, which therefore, when once put in motion by the rotation of our bodies, must continue in it for some time after this has ceased ; and to this real circular movement of the visive spirits, while the body is at rest, they attributed the apparent motions of objects in giddiness. Dechales\* saw the weakness of this hypothesis, and conjectured, that

\* *Curfus Mathematic.* Tom. ii. p. 422.

that the phenomenon might be owing to a real movement of the eyes, but produced no fact in proof of his opinion. Dr. Porterfield,\* on the contrary, supposed the difficulty of explaining it to consist in showing, why objects at rest appear in motion to an eye which is also at rest. The solution he offered of this representation of the phenomenon, is not only extremely ingenious, but is, I believe, the only probable one which can be given. It does not apply, however, to the fact which truly exists; for I shall immediately show, that the eye is not at rest, as he imagined. The last author, I know of, who has touched upon this subject, is Dr. Darwin.† His words are, “When any one turns round rapidly on one foot till he becomes dizzy, and falls upon the ground, the spectra of the ambient objects continue to present themselves in rotation, or appear to librate, and he seems to behold them for some time in motion.” I do not indeed pretend

\* Treatise on the eye, Vol. ii. p. 426.

† Philosoph. Transact. Vol. lxxvi. p. 315.

pretend to understand his opinion fully; but this much seems clear, that, if such an apparent motion of the surrounding objects depends, in any way, upon their spectra, or the illusive representations of those objects, occasioned by their former impressions upon the retinas, no similar motion would be observed, were we to turn ourselves round with our eyes shut, and not to open them till we became giddy; for in this case, as the surrounding objects could not send their pictures to the retinas, there would, consequently, be no spectra to present themselves afterward in rotation. But whoever will make the experiment, will find, that objects about him appear to be equally in motion, when he has become giddy by turning himself round, whether this has been done with his eyes open or shut. I shall now venture to propose my own opinion upon this subject.

If the eye be at rest, we judge an object to be in motion when its picture falls in succeeding times upon different parts of the retina; and if the eye be in motion, we  
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judge an object to be at rest, as long as the change in the place of its picture upon the retina, holds a certain correspondence with the change of the eye's position. Let us now suppose the eye to be in motion, while, from some disorder in the system of sensation, we are either without those feelings, which indicate the various positions of the eye, or are not able to attend to them. It is evident, that, in such a state of things, an object at rest must appear to be in motion, since it sends in succeeding times its picture to different parts of the retina. And this seems to be what happens in giddiness. I was first led to think so from observing, that, during a slight fit of giddiness I was accidentally seized with, a coloured spot, occasioned by looking steadily at a luminous body, and upon which I happened at that moment to be making an experiment, was moved in a manner altogether independant of the positions I conceived my eyes to possess. To determine this point, I again produced the spot, by looking some time at the flame of a candle;

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then turning myself round till I became giddy, I suddenly discontinued this motion, and directed my eyes to the middle of a sheet of paper, fixed upon the wall of my chamber. The spot now appeared upon the paper, but only for a moment ; for it immediately after seemed to move to one side, and the paper to the other, notwithstanding I conceived the position of my eyes to be in the mean while unchanged. To go on with the experiment, when the paper and spot had proceeded to a certain distance from each other, they suddenly came together again ; and this separation and conjunction were alternately repeated a number of times ; the limits of the separation gradually becoming less, till, at length, the paper and spot both appeared to be at rest, and the latter to be projected upon the middle of the former. I found also, upon repeating and varying the experiment a little, that when I had turned myself from left to right, the paper moved from right to left, and the spot consequently the contrary way ; but that when I had turned from right to left, the paper would then move from left to right.

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These were the appearances observed while I stood erect. When I inclined, however, my head in such a manner, as to bring the side of my face [parallel to the horizon, the spot and paper would then move from each other, one upward and the other downward. But all these phenomena demonstrate, that there was a real motion in my eyes at the time I imagined them to be at rest; for the apparent situation of the spot, with respect to the paper, could not possibly have been altered, without a real change of the position of those organs. To have the same thing proved in another way, I desired a person to turn quickly round, till he became very giddy; then to stop himself and look steadfastly at me. He did so, and I could plainly see, that, although he thought his eyes were fixed, they were in reality moving in their sockets, first toward one side, and then toward the other.

The last instance of visible motion I shall notice, is one which has been mentioned by Mr. Le Cat, in the following words:\*

\* *Traité des Sens.* p. 419.

“ Place a lighted candle at a moderate dis-  
 “ tance from a polished body of considerable  
 “ convexity, so that the image of the flame,  
 “ which is seen by reflection from it, may  
 “ appear as a small luminous point. The  
 “ experiment will succeed better, if the di-  
 “ rect rays of the flame be intercepted  
 “ from the sight. Close, after this, one  
 “ eye, and view the luminous point in a  
 “ careless way, (*en revant*) that is to say,  
 “ with the eye in a relaxed or dilated state.  
 “ The point will then be seen enlarged and  
 “ radiated. If you bring now your fin-  
 “ ger to the right of the eye which is open,  
 “ and gradually move it toward the left,  
 “ in order to conceal the luminous point  
 “ from this eye, you will distinctly perceive  
 “ the shadow of your finger to proceed from  
 “ left to right, and to pass over the point  
 “ in a direction, contrary to that which  
 “ you gave it. Should you, afterward,  
 “ move your finger back from right to left,  
 “ and in like manner, if your finger be  
 “ moved from above downward, or from  
 “ below upward, the shadow will always  
 proceed

“ proceed the contrary way. It is there-  
 “ fore manifest, that the soul must here  
 “ see objects inverted, as their images  
 “ in the eye truly are; and that it refers  
 “ impressions to those parts of the eye  
 “ where it feels them, and not to the places  
 “ from which the rays are emitted, as  
 “ it does when it possesses the means  
 “ of rectifying its judgment. Whence  
 “ does this happen? Doubtless, because  
 “ the luminous point has neither a high  
 “ nor a low, neither a right nor left side,  
 “ nor any well-enlightened object in its  
 “ vicinity, to awaken the attention of the  
 “ soul; in short, nothing which can de-  
 “ termine its judgment.”

I should scarcely have mentioned this  
 experiment, from any respect for the autho-  
 rity of its author in optics; but as Haller\*  
 seems to assent to the conclusion he draws  
 from it, that the soul sometimes sees ob-  
 jects inverted; and as the Abbot Derochon,†  
 a member of that learned body, the Aca-

\* *Elementa Physiologiæ*, Tom. v. p. 479.

† *Memoires de Physique*, p. 66.

demy of Sciences of Paris, has lately, but in my opinion unsuccessfully, attempted to reconcile it to the commonly-received principles of vision, I think it worth while to show, in a few words, that it is a direct consequence of the very doctrine Mr. Le Cat means to overthrow by its means.

It would be proper, indeed, to mention before hand, the opinion of the Abbot Derochon; but this I must, notwithstanding, omit doing, as it could not be understood without the figure by which he has illustrated it. I shall observe, however, respecting it, *first*, that it requires the side of the finger next to the eye, to be without the least illumination; whereas the experiment will succeed, whether it be illuminated or not: *secondly*, that, according to it, the experiment ought to succeed equally well, whether the image of the flame in the mirror be seen as a point, or as a surface; though, in truth, it never does succeed, except in the latter cases: *thirdly*, that the apparent shadow of the  
finger

finger is always much larger than it ought to be, were it seen by reflection, as the Abbot thinks: *fourthly*, that, while the eye, mirror, flame, and finger, remain in the same positions, the shadow seems at one time larger than at another, owing to the different degrees of relaxation in the eye; but that this, for the reason just mentioned, ought never to happen, according to his theory: *fifthly*, that, agreeably to his own reasoning, the shadow ought to move in the same direction with the finger, which is the very reverse of the fact to be explained. But as arguments against error may be infinitely extended, and as only one solution of a phenomenon can be true, the readiest way of exposing the insufficiency of others, is to exhibit that which is just.

This, in the present case, seems to lie upon the very surface of optical knowledge, and has already been given by others, of various forms of the same fact. When the image of the flame is seen in the mirror as a point, its rays must be accurately

rately collected to a focus in the retina; but when seen as a surface, this must necessarily be attributed to their focus being either before or behind it; in either of which cases, they will occupy a place upon that membrane of some assignable dimensions. In the present instance, their diffusion over a part of the retina, depends on the focus being behind it; for the eye is now, from a condition of the experiment, in a more relaxed state than it was just before, when the rays of the same object were brought there accurately to a point. The rays, therefore, which go to the right side of the enlightened surface of the retina, or picture as I shall call it, are those which enter the eye at the right side of the pupil, and its left side is formed of the rays entering at the left side of the pupil; and the like must be true of its upper and lower parts. Should we then begin to move a finger from right to left across the eye, the rays forming the right side of the picture must be first intercepted. But from the known fact, that the points of an external object are always in an inverted position,

position, with respect to the parts of the retina, by the affections of which they are suggested, when the *right* side of the picture there is effaced, the *left* side of the external object it suggests must disappear. And for the same reason, if the motion of the finger be continued from right to left across the eye, the other parts of the luminous surface in the mirror will successively vanish from left to right, and thereby furnish the appearance of a shadow passing over it in that direction.— In like manner, it may be shown, that if the finger proceeds from left to right, from above downward, or from below upward, the shadow must move the opposite way.

That this is the true explanation of Mr. Le Cat's experiment, is, I think, plain, both from its intrinsic evidence, and the following considerations:—If the mirror be brought within four or five inches of the eye, and the candle be so placed, that the image of the flame must, from the laws of reflection, be regarded as a mere point ;

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though we should now view it with the utmost care, and though there should be in its neighbourhood some well-enlightened object to awaken the attention of the soul, as Mr. Le Cat expresses it, still the seeming shadow will move in a direction contrary to the finger. For the image is now so near to the eye, that no exertion we can make is sufficient to bring its rays to a point upon the retina; the picture, therefore, upon that membrane will be formed of rays passing to a focus behind it, which is the only condition necessary for the success of the experiment. Again, if a short-sighted person should place the mirror at the distance of some feet from him, complying in other respects with Mr. Le Cat's instructions, he will constantly observe the shadow to move in the same direction with the finger. For, in his eye, the rays of the image, when at such a distance, must meet before they fall upon the retina. The right side, therefore, of the picture upon that membrane, must be composed, in this case, of rays which  
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enter the eye at the left side of the pupil. Consequently, when these are cut off, the left side of the apparent luminous surface must disappear, and the shadow be seen to move the same way as the finger, when this successively intercepts the rays proceeding from the image to the eye.\*

\* Scheiner observed a fact of the like kind (*Fundamentum Opticum*, p: 33) namely, that, if a small hole, made in any substance, be held near to the eye, and an opaque body be passed between them, from right to left, the left side of the hole will first disappear. Mr. Grey afterward took notice (*Philosophical Transactions*. Vol. xix. p. 286) that a needle he employed in this experiment was seen inverted; from which he supposed that the hole, or something in it, produced the effect of a concave speculum. Mr. Harris, however, says (*Treatise of Optics*, p. 141) that it is not the needle, but its shadow on the other side, which is seen, and is the cause of the inverted appearance. But the truth is, that the hole is to be regarded as a luminous point, the rays of which fall upon the retina before they are collected to a focus; and hence that the same appearances must be here observed as in the experiment of Mr. Le Cat. In proof of this it may be mentioned, that if the hole be placed at such a distance, that the eye may refract its rays accurately to a point on the retina, no shadow or image of the needle will be seen; that if the hole be still farther removed, and the eye be adapted to a less distance, the shadow or image will again appear, but its position will now be upright, and its motion the same way as that of the needle itself; and lastly, that, at one given distance of the hole, either no shadow will appear, or it will be seen upright, or it will be seen inverted, according as the eye may be made to assume different states with respect to its power of refraction.

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## ARTICLE II.

*On a supposed Consequence of the Duration of Impressions upon the Retina; and the Effects of accurate Vision being confined to a single Point of that Membrane.*

FEW things, at first, appear more incredible to a person, not conversant in optics, than that he does not, at any one time, see distinctly a surface larger than the head of a pin. After he is convinced, by proper trials, of the truth of this, he naturally asks, Whence comes it then, that, in ordinary vision, I seem to view distinctly so many objects at once? I go into a crowded street, and I fancy I have an accurate perception by sight, of men, houses, carriages, and many other things, all at the same time; whence proceeds this illusion?

Only one answer, as far as I know, has been given to this question. The impressions made upon the retina by external objects, do not, it is said, immediately cease

cease, along with the reception of the rays which flow from them; and, as in the ordinary mode of vision, the eye is continually passing from object to object, the impression left by a former one may be still vivid, though the eye be directed to another; and hence we may imagine we see both of them distinctly, though the picture of only one occupies that place of the retina, which alone furnishes us with accurate vision.

There are, however, objections to this answer, which seem to me insurmountable. For, *in the first place*, as the duration of impressions on the retina must be greater or less, according to the vivacity of the pictures which occasion them, it follows, that, were this answer just, the apparent field of our distinct vision ought to be in proportion to the quantity of light admitted by the eye; that it should be contracted, therefore, by every cloud which passes over us, and be enlarged by every burst of sunshine; that, at mid-day, it should possess its greatest extent, and

ought from that time gradually to decrease till the evening, when its limits should be nearly the same with those of the real field of accurate vision. *Secondly*, since the coloured spot, which is produced by looking steadily for some time at a luminous body, appears projected upon every object to which we direct our eyes, during its continuance, and as such a spot is necessarily the sign and effect of the duration of an impression upon the retina; every other visible appearance from the same cause ought, in like manner, to have its situation determined by the position of the eye, as far as this may be occasioned by the action of its muscles. No object, therefore, ought to appear separate and distinct from others, if the answer were true which I am combatting; but, on the contrary, all those to which we successively direct our eyes during the limits of the duration of an impression upon the retina, should seem crowded into one place; and, consequently, none of them should be perceived with any tolerable accuracy.—

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Such are the conclusions from the truth of this answer. I need scarcely mention, that they are contradicted by experience.

There is another form of the same fact, to which, it may be thought, an explanation taken from the duration of impressions on the retina will better apply; I mean the appearance of a fiery circle, when any red-hot body is moved quickly round. But it seems, to me, that such an explanation cannot even here be admitted. For, if the circle depended upon the cause I have mentioned, it could only be observed as long as the impressions upon the retina were also disposed in the form of a circle. Were this broken upon, which it must be by every movement of the eye, the appearance suggested by the last impression would no longer be so arranged, with respect to the appearance suggested by the present impression, as to lie with it in the circumference of a circle; and hence some very different figure would be observed. Every person, however, may easily convince himself, that the circular form of  
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the fiery appearance is equally perceived, whether the eye be at rest, or be moved in the most irregular manner.

If these arguments be thought sufficient for the purpose I had in view, it must also follow from them, since the fact still remains to be explained, why we apparently see so many objects with equal distinctness at once, that past impressions upon the retina are perceived as present, by means of some higher faculty than that of sight. This faculty cannot, with propriety, be named *memory*, as it is essential to a thing's being remembered, that it be perceived as past. Nor can it be called *imagination*, since we believe in the present existence of what it perceives. In one point of view it may seem rather a defect in our natures, that we should not be able to distinguish between things past and present. However this may be, I am inclined to be of opinion, that many other phenomena, both of thought and external sense, are partly to be resolved into the same general fact. From the present instance of  
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it, we learn, that several muscular actions may be performed, in succession, during the least perceptible portion of time.

The question I have just treated, naturally gives rise to another: Would it have been more to our advantage, if accurate vision, instead of being confined to one point of the retina, had been possessed by every part of that membrane? I answer, I think not, for the following reasons.

*First*; The diffusion of such a property, over the whole retina would be of little use, unless our power of attention was also increased. For we would otherwise be still unable to perceive more than one visible object at once, with distinctness, since, by our present constitution, we are capable of attending accurately to only one thing at a time. The only benefit, indeed, I can see to arise from such a condition of the retina, is this, that our attention might be shifted more quickly from picture to picture on that membrane, than our eyes can be turned from one external object to another.

another. This advantage, however, would be far out-weighed by an inconvenience accompanying it. For it is a well-known fact, with respect to perception, that we are capable of attending, more or less accurately, to any particular impression upon the senses, in proportion to the force of the other impressions, which are at the same time received. But in the supposed state of the retina, there would be, almost always, several impressions of the same strength as the one to which we might desire particularly to attend; whereas, in its present state, the vivacity of the impression from the object, to which we turn the optic axis, most commonly surpasses, considerably, that of every other upon the same membrane; by which means our attention is rendered less liable to interruption.

*Secondly*; The extension of accurate vision, to every part of the retina, would deprive us, in great measure, of the help, which we obtain, at present, from the eye, in learning the thoughts of other men.

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As far as I have been able to observe, the changes produced by our internal feelings, upon the state of the eye itself, are very few, and relate only to the quantity of moisture, which is diffused over its surface, and the degree of fulness in the blood-vessels, which are spread upon its white and glistening part. Both of these circumstances, however, are similarly altered by opposite passions, and, consequently, neither of them can be regarded as the appropriate expression of any. The whole variety, then, of the expressions of feeling, which are justly attributed to the eye, must, I think, depend upon its motions. Some of these are the immediate effects of certain passions; the eye, for instance, being moved differently in anger and in grief; and such may be esteemed as directly expressive of the passions by which they are produced. But the far greater number of them do little more, than merely point out the external cause, or object of the sentiment, which the changes of other parts of the countenance declare to

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exist within us; or distinguish certain external appearances depending upon a mental cause, from similar appearances arising from a different source. Thus, blushing is often distinguished from an accidental flush of the cheek, by the eye being turned away from the person who occasions it.

That many of the expressions, which we attribute to the eye, do in fact depend on changes in other parts of the countenance, is evident from the alterations we think induced upon it, by the eye-lashes falling off from disease, by a slight inflammation of the edges of the eye-lids, without its being communicated to the eye itself, by artificially colouring the eye-brows, and by many other similar circumstances. And how essential to the right understanding of the expressions of the other features, are the motions of the eyes, when conducted with design, and properly directed, must be known to every one, who has attended in discourse to the countenances of very short-sighted people, and more especially to those of persons afflicted with  
 blindness

blindness from a *gutta serena*, in which the eye, with respect to its external condition, seems without fault. But whatever is the assistance the motions of the eye afford, in expressing our internal feelings, the whole of it must ultimately be referred to the circumstance of accurate vision being confined to one point of the retina; since the intent of those motions is, to bring the pictures of external objects upon the most sensible part of that membrane. Their necessity, therefore, would no longer exist, if the same property were extended, and the advantages we at present enjoy from them would, consequently, cease.

## ARTICLE III.

*On the Connection between the different refractive States of the Eyes, and the different Inclinations of the Optic Axes to each other.*

I HAVE mentioned, in my Essay upon Single Vision with Two Eyes,\* that I had been convinced, by experiments almost without number, that every different degree of the mutual inclination of the optic axes, is attended by a different state of the refracting power of each eye. The experiments I there alluded to were chiefly of this sort. I placed a luminous point, most commonly the reflected image of the flame of a candle from the bulb of a small thermometer, at such a distance, that when both my eyes were accurately directed to it, its visible appearance to one of them was likewise that of a point. Keeping then the axis of this eye fixed, and making the other to cross it, sometimes before and sometimes behind the luminous point, I found that in both

\* P. 82.

both cases it appeared as a surface to the eye, in the axis of which it was situated; and that the more remote from it was the concurrence of the axes, the larger was the luminous surface. Now when the axes met before the point, the apparent surface must have been occasioned by the rays coming to a focus, previously to their incidence upon the retina; because, when I passed my finger across the eye by which it was seen, its parts disappeared, in an order corresponding to the direction in which the finger moved. The disappearance of the parts was in an order, contrary to the motion of the finger, when my optic axes intersected each other beyond the point; which is an equal proof, that the rays, in that case, tended to a focus behind the retina.

One application of this fact has already been shown,\* and I shall now proceed to mention several other phenomena in vision, which it may serve either in whole, or in part, to explain.

1. It

\* Essay upon Single Vision, p. 33.

1. It accounts for the following beautiful observation made by Aguilonius,† that if we close one eye, and look with the other at an object placed in its own axis, we will not be able to see this object distinctly, unless we also direct to it the axis of the closed eye. For in persons, who are neither presbytic nor myopic, the refractive states of the eyes are so adapted to the mutual inclinations of the optic axes, that pencils of rays flowing from bodies at moderate distances are more accurately collected upon the retina, when they are situated at the intersection of those lines, than if their position was, in any considerable degree, either nearer or more remote. The reason given by Aguilonius himself, is, that the mind perceives only those objects distinctly, which are placed at the concurrence of the optic axes. But the following experiment proves that the solution is true no farther, than as it coincides with the one I have advanced. Hold, in the axis of either eye, a concave lens, at  
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† Aguilonii Optica, p. 24.

such a distance, that the letters of a book, placed a little farther off, may appear through it very indistinct to that eye, when both axes are directed to any particular word. View afterward the lens itself with both eyes, and the letters will immediately become more distinct. In this experiment then, an object is more accurately perceived when distant from the concurrence of the optic axes, than when situated exactly in it.

It may be said, perhaps, that the distinctness of the letters is here to be attributed to the contraction of the pupil, which is occasioned by the eyes being directed to a nearer object than they were formerly. But that this is not the case, may be made evident by another experiment: Place a convex lens in such a manner before one eye, that the flame of a candle, at the distance of two or three feet from the face, may appear indistinctly terminated to that eye, when both axes are pointed to it. The same eye being kept fixed, let the two axes afterward meet  
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beyond the flame, and it will now be seen much better defined, though the pupil is at the same time become larger. The insufficiency of the explanation of Aguilonius, is also proved, by a circumstance frequently noticed in persons who are very short-sighted; for such are observed, when they desire to view an object with much attention, to hold it close to one eye, and to turn the other aside; in this way occasioning the two axes to meet very remotely from the object.

2. The reason commonly given, why short-sighted people view an object with one eye only in the manner above-mentioned, is, that by this means they avoid the uneasy straining of the muscles, which must be employed to direct both axes to the same point. But it is evident they must derive from the practice this farther advantage, that, as their optic axes are now parallel to each other, or nearly so, they, consequently, see the object in the least refractive state of their eyes. Pencils, therefore, will now have their focuses in the retina,

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the rays of which would have crossed each other, before they fell upon it, had both the axes been directed to the object.

3. Spectacles were long employed, before the manner in which they assisted sight was known. About the year 1601, this was proposed as the subject of a question to Kepler,\* by his principal patron at that time, Ludovic L. B. a Dietrickstein, a learned nobleman of Austria. The first answer he gave was, that convex glasses were of use, by occasioning objects to appear larger. But his patron observed, that if objects were rendered by them more distinct, because larger, no person would be benefited by concave glasses, since these diminish objects. It was not till three years after, that, in consequence of finding out in what manner vision is performed, he was able to give a just solution of this problem, though his attention had been directed to it during the whole of that interval. According to the discovery he then made, convex glasses were said by him to assist the sight of pres-

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\* Paralipomena in Vitellionem, p. 200.

bytic persons, by so altering the directions of rays diverging from a near object, that they shall afterward fall upon the eye, as if they had proceeded from a more remote one; and concave glasses to benefit the myopic, by producing a contrary effect upon rays which diverge from a distant object. Now it is manifest, that by this theory, to which I believe no addition has been made by any succeeding writer, precisely the same effects are attributed to lenses, whether they be employed singly, or in the form of spectacles. I am inclined, however, to think, that a difference, sometimes at least, exists here, which has hitherto escaped notice. For in regard to such spectacles as I have tried upon myself, I have always found, that, when I looked with them at objects placed at moderate distances directly before me, my optic axes passed through the glasses, more inwardly than their centres. With respect, therefore, to spectacles for long-sighted people, as the inner halves of their glasses may be regarded as two prisms, whose refracting angles face each other,

other, to have allowed both my eyes to receive through them pencils of rays from the same point of an object, the intervals of my pupils must have been less than was necessary for that purpose in naked vision. The consequence of which would be, an increase of the refractive power of my eyes. Again; as the like parts of glasses in spectacles for short-sighted persons, may be esteemed to be two prisms, the refracting angles of which are turned from each other, the interval of the pupils must have been increased, and the refracting power of my eyes by this means diminished, when I looked at an object through them, which was directly before me. And effects similar to what I have mentioned, must have followed my viewing objects placed obliquely, through glasses of both kinds. Here then is one advantage, which persons, who see with both eyes, either do or may enjoy from spectacles, but which they cannot derive from using single glasses. For if they are presbytic, they can see an object by the means of them with a higher refractive state

of the eyes, than if the optic axes met there, as in naked vision ; and if myopic, with a lens. It is also worthy of remark, that this advantage does not ultimately tend to increase the evil, which first gives occasion for spectacles. On the contrary, if what every writer upon vision asserts be true, that we are apt to become short or long-sighted, according as we are much accustomed to view near or distant objects, it must serve to diminish that evil. In support of this opinion, I shall mention a fact, with which I have been made acquainted by Mr. George Adams,\* of this place, who is not only well skilled in the theory of vision, but, from his situation, as an artist, has better opportunities, than most persons, of learning such matters. The fact is this, that he does not know a short-sighted person, who has had occasion to increase the depth of his glasses, if he began to use them in the form of spectacles ; whereas he can recollect several instances, where those have been obliged to change their  
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\* Mathematical Instrument Maker to the King.

concave glasses repeatedly, for others of higher powers, who had been accustomed to apply them to one eye only. This indeed may have happened by accident; but, at any rate, the fact is worthy of farther attention and inquiry.

It would seem, however, that the long-sighted derive more benefit from the alteration in the mutual inclinations of the optic axes, which is produced by spectacles, than the short-sighted. For, as the inner halves of the convex glasses are to be regarded as prisms, with their refracting angles continually increasing as we approach their edges, if two objects, situated at different distances, be viewed successively through them, the inclination of the optic axes to each other, when the nearer object is seen, must bear a higher proportion to their inclination, when we look at the one more remote, than the different inclinations of the optic axes do to each other, when they are successively directed to the same objects, without the intervention of such glasses. Hence the nearer the object is,

is, the greater will be the effect of the variation in the inclination of the axes produced by spectacles with convex glasses; which is the order of things, the best adapted to the wants of those who use them. But with respect to short-sighted persons, since the refracting angles of their glasses, considered as prisms, *decrease*, in proportion as the objects seen through them become more remote; they must, consequently, derive the least benefit from an alteration in the mutual inclinations of the optic axes occasioned by their spectacles, at the time they most require it.

If it were asked, then, what is the real foundation of the common reproach against spectacles for long-sighted people? I should answer, a very different one from that, which is, for the most part, assigned.— For the change, in the conformation of the eyes, which renders them useful, seems to be one of those which nature has destined to take place at a particular age, and to which there is no gradual approach through the preceding course of life. A person, for instance,

instance, at forty, sees an object distinctly, at the same distance that he did at twenty. When he draws near to fifty, the change I have spoken of commonly comes on, and obliges him in a short time to wear spectacles. As it proceeds, he is under the necessity of using others with a higher power. But, instead of supposing that his sight is thus gradually becoming worse, from a natural process, he attributes the increase of the defect in it to his too early and frequent use of glasses. Upon the whole, I should draw this inference from what has been said, that no person, whose sight begins to grow long, ought to be, in the least, prevented from enjoying the immediate advantage which spectacles will afford him, by the fear that they will ultimately injure his eyes; not that I think the convexity of each glass, considered by itself, can do no harm, but that I believe the benefit, arising from the combination of the two, to be at least sufficient to compensate it. Whether those, who have a tendency to short-sight, should be also early in their employment of spectacles,

ſpectacles, I ſhall not pretend to ſay; as there is not the ſame ground, from theory; for ſuppoſing, that the benefit ariſing from the combination of the two glaſſes is able to over-balance the injury, produced by the concavity of each conſidered ſeparately.

All that I have ſaid, however, upon the ſubject of ſpectacles, proceeds upon the ſuppoſition, that, when objects, placed directly before us, at moderate diſtances; are viewed through them, the optic axes penetrate the glaſſes more inwardly than their centres. But I can be by no means ſure, that the interval of the pupils of other perſons bears the ſame proportion; to the interval of the centres of the lenſes in ſpectacles, as that of mine does. It concerns thoſe, therefore, who are chooſing them, to have attention to this circumſtance. To me it appears proper, that the glaſſes in ſpectacles, both for long and ſhort-ſighted people, ſhould be ſo far aſunder, that, when we look at a very remote object directly before us, our optic axes may paſs exactly through their centres. For if the  
centres



centres of convex glasses be nearer to each other, very remote objects will appear double; and if they are more distant, though the object viewed be infinitely far from us, the optic axes will, however, be inclined to one another, and the refractive power of the eyes increased; when this may be of disservice; since there are few eyes which are not able, even without the aid of the convexity of a glass, to bring parallel rays to a focus upon the retina. If the centres of lenses in spectacles, for the short-sighted, be less distant than what I have mentioned, the optic axes must be bent toward each other, when very remote objects are seen, and the refractive state of the eye, therefore, heightened, which is the very reverse of what is here to be desired. Should the interval of the centres of those lenses be greater, objects at very considerable distances will be seen double.

There are two other observations relative to glasses for the sight, which I wish to add to what I have already said upon this subject. The first is, that the single con-

vex glasses with which some persons read, must be very injurious, if they be sufficiently large, to admit the same object to be seen with both eyes. For as both axes will then pass through them, one on each side of the centre, the interval of the pupils will be widened, and the refracting power of the eyes, be diminished; so that here a disadvantage is to be added to the prejudice of the convexity of the glass, not a benefit to be placed against it, as in the case of common spectacles for the long-sighted. If, indeed, the defect in sight does not arise from the conformation of the eye, but from a want of transparency in its cornea or humours, then such glasses, by magnifying objects, will be useful, for the same reason, that, in a very faint light, we can read a book of a large print, with more ease than one of a smaller. The second observation is, that if flat-sided prisms were fixed in spectacle-frames, with their refracting angles toward each other, they would assist the long-sighted somewhat, without producing the evil which is said to arise

arise from the convexity of lenses ; and spectacles of this kind might, with more propriety, I think, than any others, be called *preservers*. A like combination of such prisms, but with their angles turned the other way, might, when the object was moderately distant, be of service to the short-sighted. But objects, very remote, would be made by them to appear double.

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#### A R T I C L E IV.

*On the Limits of perfect or distinct Vision,*

DR. Jurin,\* I believe, was the first who distinguished between *perfect* and *distinct* vision ; confining the former term to those cases, where the rays of a single pencil are collected to a single point of the retina ; and marking, by the latter, the perception we have of visible objects, when the rays of the pencils, diverging from them, though not collected to single points of

\* Essay on distinct and indistinct Vision.

the retina, yet occupy so small portions of it, as to allow the objects to be distinctly seen. But as few authors have adopted this division, I shall, in the present article, use both terms in the sense, which he has appropriated to the first. Neither of them is indeed free from objection, since bodies to be distinctly or perfectly seen, not only require, that their pictures should be accurately formed upon the retina, but that they should fall upon a particular part of it.

Although it has long been a subject of inquiry, within what limits of distance objects are distinctly perceived by sight, yet the only experiments I have met with in books, which have been made, with any tolerable show of accuracy, to determine this matter, are those of Dr. Porterfield. I shall not here say what they were, as his Treatise is in every body's hands, but shall only mention, that the principal conclusions which he drew from them were, *first*, that objects could be distinctly seen by him, that is, the pencils of rays  
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which came from them could be accurately collected to points upon the retina, when their distances from his eye did not exceed twenty-seven inches, and were not less than seven; and *secondly*, that, as often as the axes of both eyes were directed to any one point, situated within those distances, the rays proceeding from it had their focus in each retina.

As the results of some experiments, which I have made upon the same subject, differ from these conclusions of Dr. Porterfield, I have read over what he has written upon the matter with more than ordinary attention, and I think I can thence show reason, why they should not be received without caution. For, *in the first place*, his experiments are related so circumstantially, and with such an appearance of accuracy in the making of them, that you would scarcely suppose he left the least possible room for error. And yet, after finishing his account of them, he tells us, that he would have repeated them *with more care and exactness*,\* had he not  
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\* Treatise on the Eye, Vol. I. p. 423.

been interrupted. *Secondly*, his experiments were made upon one eye only, though his conclusions apply to both eyes; an inaccuracy which gives occasion to suspect others. *Lastly*, he says, that he could not see an object distinctly at the distance of seven inches, unless both axes were pointed to another object, at only half that distance. Had he then directed both axes to an object seven inches distant, which he does not mention he ever did, it must consequently have been seen *indistinctly*; and yet one of his conclusions states, that objects, distant from about seven, to about twenty-seven inches, were always *distinctly* seen, when the axes of both eyes were directed to them. Such are the reasons which lead me to think, that the whole of the difference, between the results of the experiments of Dr. Porterfield and myself, is not to be attributed to a difference in the structure of our eyes.

The experiments, which I made upon this subject, were with luminous points. They proved to me, *first*, that, when both  
 optic

optic axes are directed to any object, placed at a less distance from my eyes than about seventeen inches, my vision of it by the left eye is indistinct, from the rays of light tending to focuses behind the retina; *secondly*, that my vision by the same eye is perfect, if the object seen, and to which both axes are turned, be from about seventeen to about nineteen inches distant; *thirdly*, that the vision of my left eye becomes again imperfect, if the object be moved to a greater distance than that of nineteen inches, the rays being now collected to focuses, previously to their falling upon the retina; and *fourthly*, that I have, by my right eye, imperfect vision of all objects, to which I direct both axes, unless their distances be so great, that the rays of each pencil, proceeding from them, may be regarded as parallel.

A conclusion is furnished by these experiments, similar to one, which was drawn by Mr. Delahire, \* from some made by himself; namely, that each eye sees objects  
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\* Mémoires de Mathématique et de Physique, 4to, p. 298.

distinctly only at one distance ; as I take for granted, that, in every case of ordinary vision, both axes are directed to the object which is viewed. But Mr. Delahire drew a second conclusion from his experiments, which he seems to have regarded only as another expression of the first, but which, in truth, includes a very different fact. It was, that the refractive state of the eye is always the same, whether we look at a very near or a very distant object. The following observations, however, will prove the contrary, at the same time that they show, in what I farther differ from Dr. Porterfield.

1. Though an object, to which both axes are pointed, does not appear distinct to my left eye, unless it be from about seventeen to about nineteen inches distant ; nor to my right eye, unless it be at a very considerable distance ; yet I find, that when the axes are made to meet at a point, about two inches distant from a line connecting the two pupils, which however cannot be effected without much straining, my left  
eye



eye will now see an object distinctly, which is only about seven inches from it, and my right eye will at the same time see an object distinctly, the distance of which is about ten inches. I find also, that my left eye is made to see an object] distinctly, though placed more than nineteen inches from it, if I direct both axes to a point still more remote.

2. I formerly mentioned, that every degree of the mutual inclination of the optic axes is attended, by a particular state of the refracting power of each eye. But I must now remark, that these states are sometimes subject to slight variations, while the inclinations of the optic axes to each other remain the same. For I find, that, when a luminous point, to which both axes are turned, is distinctly seen by my left eye, I can, by certain efforts not easily to be described, but without changing the position of either axis, make it afterward appear as a surface, and this too, at one time, from the rays coming to a focus too soon, and at another, too late, for perfect

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vision

vision.\* One instance of these variations deserves to be minutely described, as it proves, that the refractive power of the eyes is subject to greater changes, than what are shown by any experiments I have met with in authors. When I look attentively at a bright star, with the optic axes parallel to each other, it appears to my left eye a surface of some extent, and to my right eye, though not a point, yet a surface of very small extent, as small as the sphericity of the cornea and crystalline, the various refrangibility of the different kinds of light, and the width of the pupil at night, can be supposed to allow; for I find, that, if I now pass a needle across the axis of the right eye, its shadow will not be seen, But should I,  
after

\* The variations, however, seem produced in such a manner, that the middle of the set belonging to one degree of the mutual inclination of the optic axes, is always different from the middle of the set belonging to another degree of their inclination; and that, when no other effort is made, than to direct both axes to the same object, the eyes always assume the middle state of the refractive power, which accompanies that particular inclination of the axes. No argument, therefore, can hence be derived against the applications I formerly made of the general fact, respecting the connection of the refractive states of the eyes with the mutual inclinations of the optic axes.

after this, withdraw my accurate attention from the star, and view it in the state of sight we have, when we are said to be in a *reverie*, in which, though our eyes are open, we are yet scarcely conscious of seeing surrounding objects, the appearance to the right eye expands itself, and if a needle be again passed before this eye, its shadow will be observed to move over the star, in a direction contrary to that of the needle itself; a sure indication that the rays of light now tend to a focus behind the retina. In the same state of things, the appearance of the star to the left eye contracts, and if a needle be held before the eye, no shadow is seen; a sign that the rays are collected to a focus on the retina; whereas they had formerly crossed one another before they reached that membrane.

Upon the whole then it is manifest, from the experiments I have related, that my left eye can collect to focuses in the retina, rays which proceed from objects at every distance whatsoever, not less than seven inches; that my right eye can collect to fo-

cuses in the retina, rays which proceed from objects at every distance whatsoever, not less than ten inches, and even such as are somewhat convergent, since it can make those, which are parallel, to meet before they fall upon the retina; and lastly, that, while both the optic axes are directed to a point within the limits of distinct vision, the rays proceeding from it are never accurately collected to focuses in both retinas, and scarcely ever to a focus in either retina. These are likewise the principal circumstances, in which my experiments differ in their results from those of Dr. Porterfield.

In making such experiments with luminous points, one or other of two appearances very constantly occurs, neither of which, as far as I know, has been spoken of by any preceding author. The most proper way of mentioning what they are, is, perhaps, to show what ought to happen in those situations, in which they are observed.

When a beam of white light passes, obliquely, from one medium into another of different refractive power, its variously coloured rays must begin to diverge from each other, at the point of the beam's incidence upon the latter medium. In achromatic telescopes, the mutual separation of these rays is checked, and its farther increase prevented, before it becomes perceptible to sense, by the contrary refractions which they undergo, from passing, successively, through the different parts of the object-glass. Hence, some have imagined, that, since objects, in ordinary vision, are seen without colour, as far as this depends on the refractions of the eye, nature has furnished us with an instrument, constituted upon principles similar to those of the object-glass of an achromatic telescope. But every one, the least acquainted with the structure of the eye, must know, that this cannot be the case, as the refractions in it are all made one way.\* And there are experimental proofs

\* There are indeed some exceptions to this, but not of sufficient consequence to affect the present argument.

proofs, that compounded light is always separated into its parts, by passing through the eye. For if we interpose any opaque substance between us and a luminous body, so that only a very small portion of this may remain visible, it will appear to consist of three differently coloured parts, red, yellow, and blue. The reason, therefore, of objects being, for the most part, seen colourless, must be elsewhere sought.\*

Now let us suppose, that a luminous point is the only object which is seen at any one time ; should the focus of its mean refrangible rays be anterior to the retina, the middle of its picture upon that membrane must be chiefly composed of the less refrangible rays ; and this must be the reason, that, when I look attentively at a bright star with my left eye, the centre of it always appears of a light orange colour. As the beams, however, from the luminous point, which enter the eye near to its axis, suffer  
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\* Dr. Maskelyne has very learnedly treated this subject in the Philosophical Transactions, Vol. lxxix. part 2.

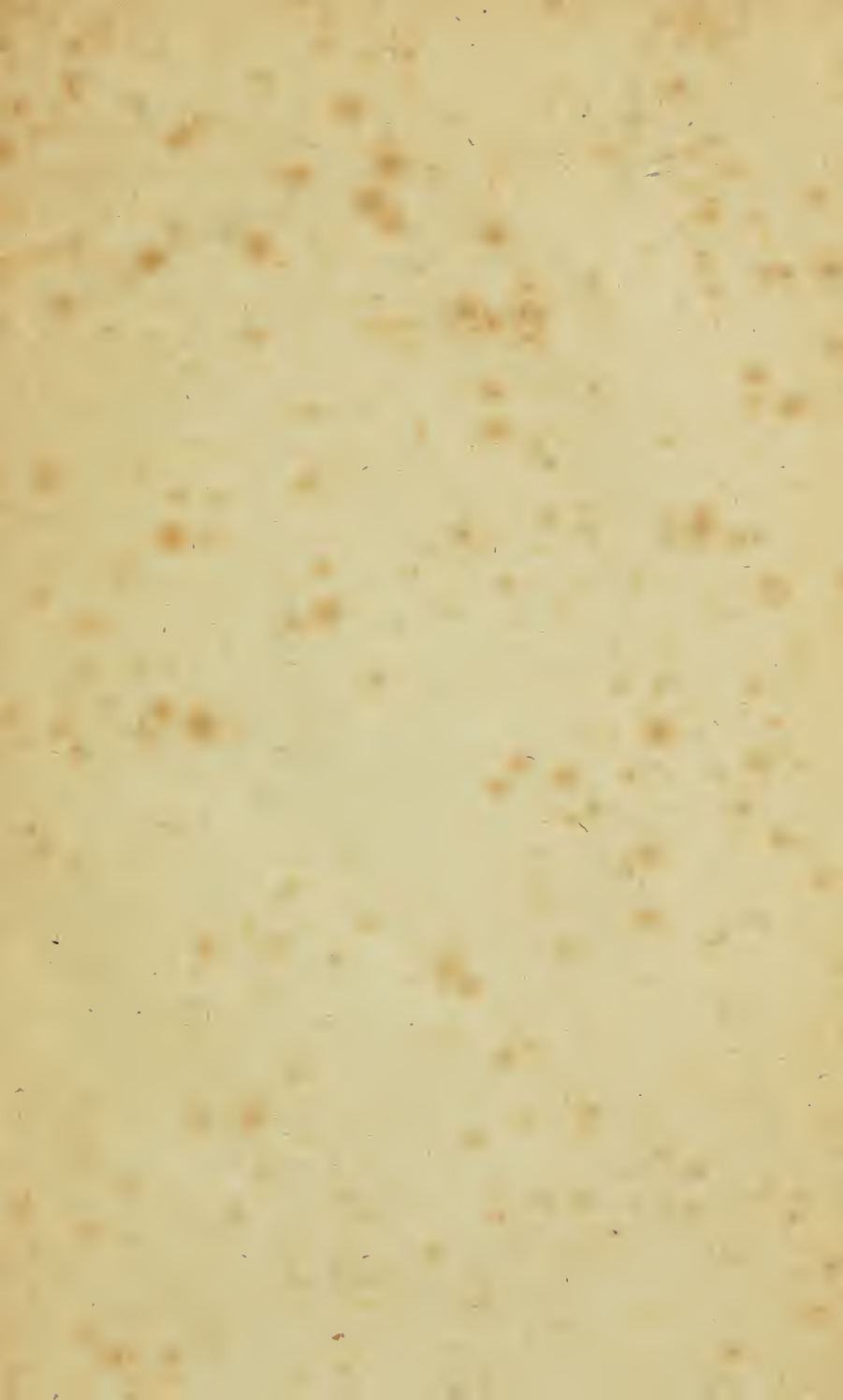
but little refraction, the brightness of their white light, will, in great measure, overpower the colour given to the middle of the picture upon the retina, by the less refrangible rays of those, which enter the eye at a distance from its axis. Were you then to intercept the former beams, the effect I have mentioned of the latter, must be more observable : and hence it is, that when I place a pin or needle between my eye and a luminous point, the rays of which come to a focus before they fall upon the retina, the shadow, instead of appearing black, is always of a red or deep orange colour ; which is one of the phenomena respecting luminous points, to which I have alluded.

On the other hand, should the focus of the mean refrangible rays of a luminous point lie behind the retina, the middle of the picture there will be principally formed of the more refrangible rays ; and if the beams, which enter the eye near to its axis, be also in the present case intercepted, the effect of the latter rays, in giving colour

to the middle of the picture, will consequently be rendered more evident. Hence it is, that, when a luminous point is not sufficiently remote for distinct vision, the seeming shadow upon it, occasioned by any small opaque object held before my eye, is always blue; and this is the second of the appearances, which I said are frequently to be observed, in experiments upon luminous points.

THE END.







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